PNEUMONIA:

ITS SUPPOSED CONNECTION,

PATHOLOGICAL AND ETIOLOGICAL,

WITH

AUTUMNAL FEVERS;

INCLUDING

AN INQUIRY INTO THE EXISTENCE AND MORBID AGENCY

OF

MALARIA.

BY

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TO CHARLES D. MEIGS, M.D.

Dear Doctor:—

To no one more appropriately than to yourself can I dedicate this volume. The innumerable marks of friendly, and indeed affectionate regard, I have received at your hands, and the devoted professional care you have taken of me and mine, by day and by night, during a period extending over more than a quarter of a century, and amid many painful trials through which a kind Providence has enabled me to pass, entitle you to this trifling return. May I trust you will receive it as a token of the sincere affection I entertain for you? Greatly do I wish I could know, or even think, that the volume which I here place before you were fully worthy of your acceptance. Before one line of it was prepared for publication in its present form, I had decided, as by the result of an instinctive impulse, to enhance its value by placing your honoured name at its head; and no one who can appreciate the feeling which prompted me, will needs be told that I have striven on that account to render the book what it ought to be. The effort may possibly—I fear will certainly—be thought by intelligent medical readers to have proved abortive. The results of my thoughts and researches, of my serious reflections and careful observations, may perhaps fail to meet the expectations of even the few indulgent professional friends who were apprised of my intention to appear before the public in this, to me, new capacity. But, should my volume meet the fate which many like it have suffered, and many more deserve to suffer, at the hands of the tribunal to whose decision all productions of the kind must be submitted—should it be soon consigned to oblivion—I shall feel neither disappointment nor regret, provided I can be certain that you have received some gratification at my having selected you as its sponsor. Would it could prove the worthy rival of the great, and unfortunately unfinished work of

our common illustrious friend, the late Dr. Drake, whose loss we cannot cease to deplore, and of whose reputation every member of our profession in this country must be proud. But this I could no more hope than you could expect; and I must be satisfied if my unpretending production can be read by you, and other medical men whose opinions are entitled to confidence, with some manifestation of approbation.

It cannot but be a matter of astonishment to you, who know something of my tastes and habits, and of the tardiness and even reluctance I have heretofore shown in preparing for publication materials long ago collected and arranged on a favourite and important subject, that I should so unexpectedly alter my course, and place before the public a work treating of a topic on which, two years ago, I had no more intention of writing than I have now of composing a practical treatise on your specialty. But so it is—*L'homme propose, et Dieu dispose.* Here I am laying aside the results of a labour of years, and troubling that very same public with a work of questionable interest on a subject only indirectly connected with that of my former researches. A few words in explanation of my reasons for so acting may not be out of place.

The present volume, like some better ones, has grown to its actual respectable dimensions from very small beginnings. The first step in its production was the writing of a friendly letter of a dozen pages, to a most valued friend, who holds an important position among the physicians and teachers of a distant State, in review of a clever essay by him, on Pneumonia. In the course of a correspondence which ensued on the subject, I prepared and forwarded another and much longer letter, in which some of the views set forth by him were critically examined. Soon after, those communications were, at the suggestion of several friends, converted into Essays, and published in successive issues of the *Charleston Medical Journal.* There they attracted the attention of some physicians, whose views on the various topics treated of coincide with mine, and were noticed with commendation in a few medical periodicals. From various quarters, I received the advice to publish them in a collected and more permanent form. I shall not stop to inquire whether this advice was impartial, or whether it was not rather founded on an unmerited estimate of the importance of the Essays, resulting from the disturbing influence which personal attachment is so apt to create under circumstances of the kind; for on a question con-

cerning the value of my own production, I leave the decision to others. But, however this may be, after some hesitation, I yielded, perhaps unwisely, to that advice, and lost no time in preparing the whole for the press—cutting out, adding, rearranging, and often rewriting. I am not sure that by these changes I have improved the original publication. On that score, indeed, I have had misgivings, and at times entertained serious thoughts of consigning the whole to the flames. But again I submitted to the decision of those in whose judgment I repose confidence. Encouraged by their opinion, I dare hope that if, by acting as I have done, no improvement in the original text has been effected, I shall, at least, not be taxed with having augmented the many blemishes it already contained.

While entertaining this hope, I cannot help fearing that my professional readers, and yourself among them, will disapprove of the result of my labours, when it is perceived that, instead of curtailing the many redundances, omitting the facts of doubtful importance, abridging the lengthy statements, and expunging a large amount of the references embodied in the original publication, I have added fresh materials to the stock. Not less do I fear that I shall be censured for not having taken adequate pains to elaborate the whole, to class properly and clearly the various topics examined, to avoid enlarging unduly on some points, and slurring over others, as well as to correct and polish the language.

But although ready to admit that in these matters the volume is amenable to the censure of tasteful readers, and fairly exposed to the lash of the critic, I cannot but think that those who honour me with an attentive perusal, will abate somewhat of the severity of this judgment, when they reflect that the main object of the work was, not to produce an elegant and finished composition, but to point out the erroneousness of views entertained by respectable writers on certain pathological and etiological subjects; that this could not be more successfully done than by accumulating as many facts as possible, with a view to sustain each limb of the argument; and that, in order to impress on the mind of the inquirer an idea of the appositeness and authenticity of those facts, and to enable him to verify their accuracy, it would be advantageous to give them mostly in the language of their reporters, and to state exactly where the records of them were to be found.

In reference to the want of artistic skill displayed throughout the entire volume, I can only express my regret. No one is more

aware of the defect than myself. Nor shall I deny that the charge respecting the absence of a proper classification, and the disproportionate extent allotted to some of the subjects treated of, at the expense of others, is well founded. I feel especially that I might, without detriment, have disconnected the long inquiry relative to the existence and morbific agency of malaria in the production of autumnal fevers from the rest of the work, have given it a separate and independent form, and published it apart; or, what would perhaps have been still better, omitted it altogether; and, taking for granted—what is not far from the truth—that nearly every one admits the existence and agency of a febrile poison, have proceeded in the argument without troubling myself with the denials of a few opponents. Such might, perhaps, have been my course, but I thought otherwise at the time; and, indeed, think so still. In order to sustain the position I had assumed, it was necessary to show that pneumonia, which some writers regard as a mere form of autumnal fevers, arises from causes distinct from those to which the latter diseases are due; and it appeared to me that this could not be better done than by first inquiring what those causes really were, and then showing that, while pulmonary inflammation is produced by a certain set of agencies, the fevers in question are the offspring of a specific gaseous poison. But, at the same time, it occurred to me that, as some of the advocates of the identity of those diseases disbelieved the existence and ignored the agency of this poison, my task would be unfinished were I not to demonstrate their errors on this point. Influenced by these views, I set to work; facts, and what I may, perhaps wrongfully, consider arguments, accumulated under my pen, and the result has been the strange compound I to-day lay before you, and for the many imperfections of which I solicit your kind indulgence.

I need not tell you—for you will easily perceive on casting your eyes over the following pages—that I have not aimed at offering anything new; that it has not been my object to start a new theory or hypothesis, and to establish it on grounds, and by facts, heretofore unknown, and by the help of arguments equally unheard of. So far from this, I have no hesitation in stating that the idea of claiming credit for originality has never entered my mind. All the credit to which I think myself entitled—if any can be legitimately considered due to me—is for having collected, within a comparatively small compass, the main facts

bearing on the question at issue, derived from reliable sources in various sections of the globe, and from the results of my own personal observation; for having examined, to the best of my ability, the subject in all its bearings; for having demonstrated, in as forcible a way as I have been able, that the idea of the identity under consideration is founded on insufficient and incorrect data, and is, in fact, little more than a dream of the imagination; and, at the same time, for having proved that etiologists who regard the various forms of autumnal fevers as due to the action of particular poisons floating in the atmosphere of specific localities, have just cause for entertaining that belief. This, I repeat, is all I have attempted to perform.

Nature has, I think, given me a decided taste for certain investigations—a large share of power of application—some degree of aptness, as I have been told, to observe and reason correctly. At the same time, circumstances have often been such as to allow me leisure to indulge my inclination for research; while, during the third part of a century that I have been attached to the medical profession, my opportunities for observation have neither been few nor neglected. But to originality of thought, or the ability to make striking discoveries, I can lay no claim. Indeed, were I so gifted, I am not sure that I should not endeavour to restrain the exercise of these powers, for fear of entering into the boundless field of hypothesis. On this subject my mind has long been made up; and from all I have seen, I can entertain no doubt, that more good is to be effected by a patient accumulation and comparison of important facts, and by endeavouring to draw from the whole correct philosophical deductions, than by adopting a different course, too common among the professional writers of this country and elsewhere; who, discarding the results of the experience of former and present times, and relying exclusively on their own too often scanty observations, make up for their other deficiencies by an indulgence in theoretical explanations; and sneer at the patient, slow, and cautious observer, and the erudite student. That the tendency to the course here adverted to is displayed by a goodly number of physicians among us is, as you know full well, too true to be denied. It has long been to me, and to others on whose natural good sense, sound judgment, and medical scholarship we may rely, a source of deep solicitude; threatening, as it does, if allowed to continue unchecked, to affect injuriously our literary and scientific

character, and to retard the advancement of useful professional knowledge among us. The picture I have drawn is not exaggerated; and the history of medicine clearly shows that those who have improved the scientific and practical department of our art, have, with few exceptions, been men, not of lively imagination and inventive powers of mind, but careful and industrious observers; men of sound judgment, and of well-read and cultivated minds.

Indeed, this subject is so important and interesting, that I trust it will justify my taking this opportunity to offer a few remarks on some of the leading circumstances which have exercised an injurious influence on medical literature and professional knowledge in this country, and which it ought to be the aim of every true votary of our art to modify and remove.

Compared with that of Europe, whether of Germany, France, England, or even Italy, the medical literature of the United States may be said to be as yet in a state of infancy. This is true both as regards the number of original publications which issue from the press, and their practical importance, scientific character, or literary merit. It would be unnecessary, and out of place, to dwell here on the causes which have contributed to produce this result; but, be they what they may, that result is placed beyond the possibility of doubt; for no professional reader can have failed to perceive that, although original treatises, monographs, and elaborate essays on various branches of medical science, have appeared on this side of the Atlantic, their number is comparatively limited; and it is a fact which no one here will venture to deny that, while some of these would do honour to any country, the greater number are not of such a character as to entitle them to general commendation and lasting attention. Not less readily will it be admitted, by those who take an expansive survey of the minor medical productions of this country, such as original essays of small size, either issued in a separate form, or as contributions to periodical journals, that, although more numerous than the former class, they seldom add anything to the character of our professional literature, and exhibit even less merit, both in reference to the matter they contain, and the manner in which they are written, than might have been anticipated when we consider the multitude of physicians scattered over the vast extent of this country, the unlimited opportunities for collecting observations of interest and value within their reach, as well as the sound sense and the capacity for practical and

scientific investigation which form attributes of the American mind. With these facts before him, every American physician who is alive to the honour, dignity, and interest of his profession, will unite in the opinion that we have, so far as medical literature is concerned, a character, not to uphold merely, but to establish; and that consequently it behooves him, while neglecting no means to improve himself in the various branches of medical science, to shun carefully, and to reprove in others, every practice which may, in any degree, retard the accomplishment of so desirable an end.

It would be impossible, in the space allowed me on this occasion, to point out in detail the several baneful influences here alluded to; let it suffice to dwell on two of the more prominent. It is not uncommon to find inexperienced medical writers in this country—as, indeed, elsewhere—hazarding, on physiological, pathological, etiological, or practical subjects, opinions completely at variance with those which the enlightened portion of medical men, throughout the civilized world, regard as placed beyond the reach of cavil or disputation, or, at least, as entitled to the most respectful consideration. Some, in their dissent from the current opinion of the medical world, content themselves with throwing out a simple conjecture, or perchance pronouncing a positive statement. Others, more ambitious, bring forward a complex theory, or do not hesitate to enter upon systematic developments. Nor is it less common to find these scientific aspirants upholding their novelties by an ostentatious display of argument, by an appeal to collateral illustrations, and by a triumphant reference to facts of a more or less apposite character; the whole seasoned with a confident and uncompromising assertion of the legitimacy of their deductions, and with a decided, sometimes contemptuous, condemnation of the views entertained on the same subjects by all preceding or contemporary writers. As might be foreseen, such attempts at innovation, the detailed enumeration of which would form an amusing chapter in the history of our profession, have so far, with occasional exceptions, failed to produce the effects intended, and to command general and continuous attention.

Novel explanations of known phenomena are offered; analogies or differences heretofore unthought of, are pointed out and insisted upon; effects are ascribed to causes, which before were unsuspected of producing them; while, on the other hand, the agency of morbid influences, universally regarded as occasioning certain phenomena,

is denied; remedies are asserted to possess powers very different from those usually attributed to them; superiority of success is claimed for modes of treatment which experienced practitioners have been taught to view with suspicion; but all in vain. The reader, if not a novice in medical literature, and a tyro in professional knowledge, is not slow to discover the small degree of reliance which can be placed on such attempts at innovation; and finds that the theories or hypotheses so pompously and confidently set forth, so far from being satisfactory, and likely to answer the purpose of their authors, are generally of a loose, crude, and unphilosophical character; in many cases evidently the offspring of men who are young in years and young in experience, who are richer in self-esteem and assurance than discretion and learning; of men whose minds are immature, untrained, and ill stored. He finds that they are founded on hastily collected and ill-digested observations, cemented together by illogical or overstrained reasonings, and though fit, perhaps, for the edification of young beginners, are unworthy of serious consideration on the part of men of mature age, and sound and extensive acquirements.

Not unfrequently, the well-informed reader recognizes in the proposed novelty some old acquaintance, which—after having enjoyed, at some bygone time, an ephemeral reputation, had disappeared under the lash of the reviewer, or sunk into oblivion from an innate want of vital force—is now once more brought forth, in a more or less modified garb, to glitter for a short moment in the eyes of the unskilled, and, of course, soon to meet a fate similar to that it had already encountered. Even when he finds, in such lucubrations, something calculated to amuse or interest, or sometimes, perchance, to seduce him momentarily from the more rational opinions he had before entertained, he in a brief while becomes sensible of his error, and reverts to his former faith. If he does not forget what he has just read, he at least views it in the same light as other vagaries, of which the annals of medicine, both on this and on the other side of the Atlantic, furnish many a curious example. With such facts before him, he cannot be regarded as over fastidious if he unite in sentiment with those who maintain that the stuff of which medical reformers and leaders in scientific advancement are made is a rare product; that in all parts of the world, and here, perhaps, more than elsewhere, readiness and smartness have but too often been mistaken for strong power of thought, and superficial in-

formation has taken the place of sound and accurate learning; that in a field where men of well-disciplined and well-stored minds, and rich in accurate observation—men who have within their reach the means of testing the statements of other investigators, whose standing is equal to their own—men who live, as it were, in an atmosphere of science, have failed; it is scarcely to be expected that individuals of ordinary capacity, with little experience, and as little reading, who are unused to accurate processes of scientific investigation and close induction, and who possibly move within localities where opportunities for prosecuting the requisite researches on a sufficiently enlarged scale are, in a great measure, wanting, will reap laurels, and assume a conspicuous place in the ranks of medical reformers, or add greatly to the stock of useful knowledge.

The cause of these successive and repeated efforts at revolutionizing the science in some or all of its parts, of this reluctance to pursue the common track, and of this disposition to discard opinions long entertained, and to substitute others of a contrary character, need not be made here the subject of detailed examination. That in some cases the result is due solely, or in a great measure, to a craving after notoriety—to the ambitious desire to be pointed out by medical or rather by unprofessional men, as authors of brilliant discoveries, and as gifted agents of scientific and practical improvements; in other words, that the guiding impulse is more frequently the desire for personal advancement and pecuniary gain, than a due regard for the interest of science, we have every reason to conclude. Equally true is it, that we may sometimes trace this result to the mere desire of gratifying the innocent and harmless vanity to which some individuals, especially young medical men, fresh from the schools, are keenly alive, that of seeing their names in print; more particularly if they can so appear in connection with something really or seemingly new, and calculated to attract attention. These lucubrations, though of no earthly value to any one in a scientific or other point of view, are to their promulgators objects of vast importance; and hence the desire to see them spread out in the pages of a medical journal, side by side with the contributions of older and better known physicians. We may also suppose that, in some instances, it is the offspring of the foible which nature has infused into the composition of some happy individuals, who imagine themselves endowed with the faculty of unravelling the most intricate mysteries of the science, and of

discovering truths heretofore concealed from the notice of medical investigators from the days of Hippocrates to the present; and who fancy they can explain all professional questions, however complex and apparently foreign to their ordinary pursuits or their opportunities of inquiry, in a more lucid, natural, and satisfactory way than any of their predecessors or contemporaries.

Such is the probable explanation in many instances. But the medical world is occasionally startled by the appearance of hypotheses and theories, both crude and untenable, and sometimes of more than problematical originality, by a very different class of men. The hypotheses to which allusion is here made are promulgated with unflinching confidence by individuals to whom, from the distinguished position they have reached in the ranks of the profession, from the respectable character of their intellectual endowments, from the extent of their general and medical acquirements, no less than from the high standing of their moral character, we cannot justly attribute selfish considerations, youthful vanity, or the monomania of authorship; and who might have been expected to abstain from the attempt to stem the current of received opinions, and to aspire to the honours awarded to true medical reformers, and correct exponents of new views. In these, the cause must be sought, sometimes in the want of a proper balance between the fancy and the judgment, a defect which leads the individual to mistake ingenious flights of the imagination, odd notions, and whimsicalities, for correct inferences from enlarged and accurate observations. In others, it is to be found in a disposition, not uncommon even among otherwise clever men, to discard received opinions whenever, in their estimations, these do not prove satisfactory on all points, however shadowy may be the grounds of dissatisfaction, and to fly at once to the most opposite modes of explanation.

In a different class, we must seek the explanation in a restless tendency, not unfrequently exhibited by individuals whose minds, though naturally good and well cultivated, have, nevertheless, not been thoroughly trained in the school of severe induction, to jump hastily at conclusions and to draw inferences without having collected, analyzed, and compared a sufficient number of facts, bearing directly or indirectly on the point at issue. Sometimes we find it in a disinclination to examine the subject in all its bearings; in a deficiency in the power of appreciating the full force of facts and circumstances militating against the views adopted, or a too

habitual tendency to undervalue the authority of those by whom they are adduced. Again, it is traced to an inability to discover the connection existing as cause and effect, between the phenomena to which attention is called; and quite as frequently, to a scanty acquaintance with what has been already written on the subject on which the author proposes to enlighten the medical public.

It has been remarked by competent judges, that the first thing an individual should attend to, who undertakes to write on professional topics, especially when he fancies he has discovered new truths, or devised a theory respecting the reciprocal relations of facts already known, which is more acceptable than any anteriorly received, is to make himself familiar with all that has been written on the subject. "When the observer," says Sprengle, "whatever be the extent of his genius, has, nevertheless, not enough of erudition to be acquainted with the observations of his predecessors, he runs the risk of repeating what has already been said a hundred times before, and of publishing it as his own discovery. Hence, the advantages of true erudition."[1]

Well would it be for aspiring reformers and discoverers, abroad and at home, were they to take heed of the sage advice of the great historian of medicine. By enlarging the sphere of their professional erudition beyond a few text and other works of easy access, or the numbers of some periodical journals, they would possibly be deterred from laying before the medical world the products of their unimportant cogitations, and the results of their imperfect observations, seeing that others, differing but little from them, had been presented—perhaps more forcibly and clearly—before, and had been long refuted or disproved, or passed by without comment or sign of approbation; thus saving themselves the risk of disappointment, perhaps mortification, and sparing to others a vast amount of useless reading.

Easy as it may be to account for the manifestation of the propensity in question among the several classes of physicians just passed in review, yet instances occasionally present themselves, in which opinions of an unsatisfactory, untenable—not to say extravagant character, often of more than doubtful originality, and which, if proved to be well founded, would overturn all our previous and long established views, are thrown out, and boldly and confidently

[1] Handbook of Pathology, vol. i.

asserted, in quarters where none of the explanatory reasons above enumerated would seem to apply.

These opinions originate with individuals in whom the imaginative powers have always appeared to be properly tempered by sound judgment; who have not, in other matters, evinced a tendency to adopt hasty and far-fetched conclusions; whose field of observation has been ample, and assiduously and profitably cultivated; whose search after information on collateral branches of knowledge has never been neglected; and by whom care has evidently been taken to render themselves familiar with a large share of the writings of the best authors. These stand apart from all other innovators, and might properly be made here the subject of some remarks; but, from want of room, and the fear of exhausting the patience of the reader, which has been already too heavily taxed, I must resign into other and abler hands the duty of clearing up the mystery which this manifestation of the tendency of which I am speaking is so well calculated to create.

It is needless to illustrate the correctness of these remarks by a detailed account of instances in which the propensity in question has been indulged to a greater or less degree. Cases, in which facts and views long familiar to well-read physicians have been seriously presented as new, or cases in which the strangest vagaries have been launched forth, and strenuously maintained, will doubtless present themselves to the mind of every reader, who will find no difficulty in placing the authors of them in some one or other of the categories above enumerated.

It is important that the aspirant to professional fame, who places himself before the public in the capacity of author, should carefully avoid every attempt, or semblance of an attempt, to pass off as his own, in whole or in part, what in reality belongs to others. In other words, too much care cannot be taken by medical writers to shield themselves from the accusation of plagiarism; whether in reference to points of doctrine or to facts, or whether it applies to the language in which the borrowed materials are conveyed. Many a reputation, otherwise unassailable, has been greatly stained by a single act of literary pilfering. Such acts are of course dishonourable, and should, like every other illicit appropriation, be shunned by all men endowed with a proper share of moral sense; and unhesitatingly exposed by every medical man, who is alive to the honour

and dignity of his profession, and who necessarily feels that the commission of such acts must, if frequently repeated, and allowed to pass unnoticed, cast a shade over the reputation of the medical literature of the country, and make it an object of derision with European writers. Far be it from us to affirm that such irregularities are not committed on the other side of the Atlantic. Were this the proper place, many and curious cases in point might be collected from the productions of the English and French medical press. But on these we need not dwell. It is sufficient for us to guard our own reputation. Depredations of this sort in Europe can prove no valid excuse for their commission on this side of the Atlantic. Let European writers steal from each other, and from us, if they think fit. The theft will disgrace the culprit, but in no way reflect injuriously on the medical literature of the country at large; which is too rich in original works of merit to be seriously affected by a few transgressions of this sort. But, in the name of all that is manly and honourable, let us not lay ourselves open to such accusations. We may doubtless congratulate ourselves that thus far acts of decided plagiarism have not been exceedingly numerous among us. Yet, while doing so, it cannot be concealed that several have already been committed; that some occasionally come to light, and that a few of those detected are of a barefaced and even monstrous character, and have proceeded from quarters where they might have been least expected to originate. Let us hope that such transgressions will not be repeated, and that, if repeated, their authors will be exposed and consigned to the contempt of their professional brethren. Too much care cannot be taken to put a stop to such proceedings; for their effect on the reputation of our infant medical literature must necessarily be of the most baneful character. If the physicians of this country frequently steal from European writers, or from each other, all confidence in their integrity will be lost. He who can attempt to deceive in respect to his claims to authorship; he who can stoop to the disgraceful act of purloining the thoughts of his brethren; he who reproduces them in the same way and in the same words as the rightful owner, without acknowledgment, exposes himself to the danger of being suspected of dishonest dealings in all his subsequent literary labours; while his misdemeanors cast a stigma on the professional body to which he belongs, reflect injuriously on the scanty literature of the land, and may naturally

cause the productions of other and more honest writers to be received with caution, or to be overlooked entirely, lest they also may be a transcript of works already known.

But I have said more than I had intended, and, soliciting your indulgence for my loquacity,

I remain, my dear Doctor,

Very affectionately and gratefully,

Your friend,

R. LA ROCHE.

WASHINGTON SQUARE:
January 12, 1854.

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PNEUMONIA

AND

AUTUMNAL FEVERS.

CHAPTER I.

BELIEF IN THE CONNECTION OF PNEUMONIA WITH AUTUMNAL FEVERS LONG ENTERTAINED.

The idea of a close connection, as regards both causation and nature, between thoracic inflammations and malarial fevers of various grades and types, has long been entertained, and continues even now to be advocated by writers of respectable standing. Casually suggested, or openly avowed and sustained at various periods by professional authorities on the other side of the Atlantic, it has met with special favour in this country, particularly in our Southern and Southwestern States, where it now enlists many warm and uncompromising defenders. Singular as it may appear to modern etiologists and pathologists, that a connection of the kind should have been made the subject of serious consideration by our forefathers, and still more, that it should receive the sanction of physicians of the present age, it requires but a slight acquaintance with the medical literature of past and present times, to be aware of the reality of the fact. To those who have not directed their attention to the subject, a few references to the writings of preceding and contemporary authors may not be unacceptable.

The retrospect, if not otherwise serviceable, may do some good by contributing to open the eyes of the modern advocates of this belief, to the fact that they are treading on ground already and frequently travelled over and as frequently abandoned; and that, consequently, whatever credit they may expect for the ingenuity displayed in its support, they must sedulously avoid laying claims

to originality; while in the progress of our inquiries, enough, it is hoped, will be said to shake their belief in the infallibility of their views, and to lead them to turn their attention to objects more worthy of serious investigation. I would hope, at the same time, to guard unprejudiced readers against adopting without mature examination, all that has been or may be said in opposition to the independent existence of the aforesaid diseases.

If we open the records of medicine, and inquire into the views entertained at various times respecting the sources of febrile complaints, more particularly of that variety now classed under the denomination of periodic or autumnal, we shall find, that the supposition of their being produced by morbid agencies of a general character, but differing materially from the mere changes in the ordinary and sensible qualities of the atmosphere to which inflammatory diseases of membranous and parenchymatous parts are usually due, may be traced to a very early period, and that, passing through successive ages to the present time, it has finally assumed the shape of a distinct theory. By some writers they are attributed to various mysterious constitutions of atmosphere. Others see in them the effects of peculiar conditions of the surrounding medium—certain secret influences appertaining in a special manner to each separate season. Others, again, speak of exhalations issuing from the bowels of the earth; while another, and more numerous set, refer them to miasmata exhaled from organic matter—animal or vegetable, or both—in a state of decomposition. But although autumnal fevers—remittent and intermittent—were at a remote period, and to this day continue to be, considered as the offspring of a distinct or specific cause, or of some peculiar modification of ordinary morbific agencies, the theory has not escaped opposition. So far from this, it is not uncommon to find medical writers of former and even modern times, upholding the opinion, that thoracic and other phlegmasiæ—some forms of them, at least—arise from causes identical with, or closely allied to those that give rise to the above-mentioned variety of idiopathic fevers; or rather viewing the latter as often produced by causes known to occasion common inflammation and *vice versâ;* and as a natural consequence regarding them all as mere modifications of one and the same disease. Now, without reverting to the numerous illustrations of the latter opinion, scattered through the valuable volumes handed down to us by the physicians of Greece and Rome, but coming down at once to the seventeenth century, we find that Ramaz-

zini ascribes the apoplexies, quinsies, catarrhs, pleurisies, and inflammations of the lungs, which occurred extensively at Modena in 1691, to the same unhealthy state of the air, induced by the wet condition of the soil and subsequent desiccating action of the sun, to which he refers the intermitting fevers of spring, and the double tertians and semi-tertians of summer and autumn. In some of the epidemics described by others—predecessors and contemporaries of Lancisi—febrific exhalations are represented as giving rise to diseases which they call dysentery, apoplexy, rheumatism, and peripneumonia. Lancisi[1] himself, in tracing the history and progress of an epidemic of rheumatism which prevailed at Rome in the year 1709, ascribes it to a like cause. The disease spread extensively, contemporaneously with the usual fevers of the season. Many of the cases were characterized by inflammation of the throat, of the windpipe, the pleura, the lungs, &c., all of which are referred to the operation of the same cause which gave rise to the fever. The same Lancisi admitted that fevers occasionally arise from a cause distinct from miasmal exhalation;—the ungenial north winds, and the intemperies resulting therefrom; the very morbid influences which he recognizes as productive of pneumonic, rheumatic, and other inflammations.

By Sydenham, similar views, respecting the pathological and etiological identity of the diseases in question, were evidently entertained. To this eminent physician and accurate observer, the doctrine of the malarial origin of endemic or epidemic fevers does not appear to have been known; or if known, to have proved admissible. Sydenham speaks, it is true, of exhalations from the bowels of the earth, as the productive agent of epidemics; but, far from apportioning them to the production of autumnal fevers exclusively, and attributing other diseases to different morbific influences, he seems to view them simply as the cause of, or an adjunct, to those general modifications of the atmosphere which, under the name of medical constitutions and latterly of meteorations, have been the subject of so much comment, and to which he refers all epidemic diseases, whether of a strictly febrile character, or marked by thoracic inflammation. He nowhere attributes fevers to a special cause exhaling from the surface of the earth itself, or from extraneous substances existing thereon, in the sense referred to by Lancisi and

[1] De Noxiis Paludum Effluviis, *Op.* ii. 103.

subsequent writers. His readers will recollect the remarks he makes under the head of "Epidemics." There are different constitutions in different years. They originate neither in their heat nor their cold, their wet nor their drought; but they depend upon certain hidden and inexplicable changes within the bowels of the earth. By the effluvia from these, the atmosphere becomes contaminate, and the bodies of men are predisposed and determined, as the case may be, to this or that complaint. This continues during the continuance of this or that constitution, which, after the cycle of a few years, gives ground and makes way for another.[1] But they will recollect at the same time, that the remarks of the English Hippocrates applied to epidemics generally, as well to the epidemic cough of 1675, with pleurisy and peripneumony, as to the continued and intermittent fevers of other years. In all, the terrestrial effluvia was supposed to predispose the system to a particular form of disease, which other causes, differing but little from each other, whatever the characters of the epidemic might be, excited into action. They were all, therefore, regarded as closely allied to each other with reference to causation. Indeed, we have proof enough in the accounts Sydenham gives us of the epidemic constitutions of various seasons—of 1674–1675, for example—that he regarded the cases of pleurisy and pneumonia, which then occurred, as "really and substantially nothing more than peculiar forms" of the reigning fever. "Sometimes," he remarks, "it (the fever) attacked the head, sometimes the bowels. It everywhere put on the symptoms of the particular part affected. Such was the case till the end of October. At that time the weather, which had been as warm and as mild as summer, suddenly changed to wet and cold. This brought on coughs and catarrhs, which were more numerous than I remember them to have been. What, however, is of more importance, is the fact that upon these coughs supervened the stationary fever of the year; and this having once taken its hold, increased, and varied in some of its symptoms from the fever of the previous part of the year. The attack of the previous fever had been chiefly determined towards the head and bowels. That of the present was towards the lungs and pleura, and as such gave rise to symptoms of pneumonia and pleurisy."[2] In 1675, "the coughs paved the way to fever, and passed without difficulty into it. Meanwhile, just as

[1] Works, i. 33–4; Ed. of Sydenham Society. [2] *Ibid.* 205.

the coughs helped the constitution in producing the fever, so also was the fever determined by the coughs to the lungs and pleura. These it attacked just as, a week before, it had attacked the head. This sudden change inclined the unthinking to consider the fever as an essential pleurisy, or an essential peripneumony. Yet it was neither more nor less than what it had been throughout."[1]

Even at a time when, with the progress of knowledge, febrile complaints began to be more definitely traced to separate and special causes distinct from those occasioning parenchymatous and membranous inflammations of the lungs, some authors of repute, from whom better things might have been expected, continued to refer these—certain forms of them at least—especially when they spread extensively, and presented unusual phenomena and a tendency to assume a periodic type, to the same agencies as usually give rise to epidemic and endemic fevers. Of this we have an example in no less a man than Cleghorn, who, in his account of the bilious pleurisy which spread epidemically in Minorca during the latter part of 1745 and beginning of 1746, and forms the subject of one of the most interesting chapters of his invaluable work, expresses himself on that matter in terms that can scarcely be misunderstood.[2]

The disease commenced like an ague fit, with shivering and shaking; flying pains; bilious vomiting and purging, succeeded by quick breathing; immoderate thirst; inward heat; headache, and fever. It observed a remittent type, and on the third or beginning of the fourth day, there was frequently a great remission; sometimes a total cessation of every violent symptom.

But, on the fourth or fifth, the disease was aggravated, and the patient expired in a day or two, either suffocated or raving mad. In another place, this distinguished observer remarks that "the anniversary epidemical fevers in Minorca may be divided into two classes—the summer and winter fevers. The former break out in June and July, and cease about January, or somewhat sooner. The latter seldom appear before November, and are rarely seen after the summer solstice. Both these classes of fevers, and indeed almost all others which happen in that climate, whether primary or symptomatical diseases, may be termed periodical, having remissions at intervals more or less considerable. But those of the summer gene-

[1] Works, i. 226; Ed. of Sydenham Society.

[2] Observations on the Epidemic Diseases in Minorca, 257–261.

rally assume some one or other of the tertian types, being worse one day and better the next, alternately; whereas, the winter fevers, though they often counterfeit tertians, especially in their beginning, yet, for the most part, have exacerbations equally strong every day. It may likewise be remarked that as the summer fevers are generally complicated with fluxes and painful obstructions in the chylopoictic viscera, so are those of the winter with coughs, catarrhs, and topical inflammations of the vital organs; the brain; the *lungs;* the heart itself."

Pleurisy, therefore, according to this view of the subject, is nothing more nor less than a modification of marsh or periodic fever, or a form of the disease of which common periodic fever constitutes another form; the difference between them depending on difference of the parts pathologically implicated, and this in its turn being due to the difference of the particular atmospheric influence giving rise to the disease;—the cold of winter being the exciting cause of the one form, and the heat of summer the exciting cause of the form peculiar to that season. Indeed, we cannot discover that Cleghorn anywhere attributes pulmonary inflammations and fevers to separate and specifically different causes, or alludes to anything more than atmospheric influences, and that he regarded those diseases as essentially different in a pathological point of view. As the one declines the other appears. The former is the chief among the vernal epidemics, as the other is constantly foremost among the autumnal. They are, to all intents and purposes, one and the same disease; the only difference being in the local affections that may supervene during their course, and by which they may be complicated; a diversity itself due to peculiar thermometric changes.

Long after Cleghorn, Dr. Wells advocated the opinion of the existence of a connection between malarial fevers and inflammatory diseases of the lungs. Whether, however, this connection was thought by him to imply the existence of a close pathological and etiological alliance or identity, is a question I have not been able to find out. It was founded on the circumstance that the two diseases exist at the same time, or succeed each other; and that those who have the one disease in one season, are liable to have the other in the next. Thus, Dr. Wells was told by his preceptor, Dr. Garden, who had practised in South Carolina, that he had found that those who suffered severely in the autumn from intermittents, were the most liable to pleurisy in the spring. "Dr. Chalmers, another

physician of South Carolina (*Essay on Fevers*), has said that nothing more frequently happens in that country than the accession of an intermittent a day or two after the removal of a pleurisy. In the year 1777, I saw the remark of Dr. Garden confirmed in a regiment of soldiers, stationed in Guelderland, part of which had been quartered the preceding autumn in Zealand. In a report made by Dr. Blane, Dr. Borland, and Dr. Lempriere, to the British Government, in October, 1809, on the sickness of our troops in Zealand, it is mentioned, on the authority of the inhabitants, that such of our soldiers as had suffered from intermittents in the autumn would run the risk of being cut off by inflammatory diseases in the winter and spring. No mention, indeed, is made of the particular kinds which would then prevail; but what is said of their fatality, seems to show that inflammations of the chest were chiefly referred to; and, at any rate, it is certain that these diseases must have been included under the general term inflammatory." Dr. Wells farther appeals to Huxham (20), to show that fevers sometimes are rife and contemporary with epidemic pleurisies and peripneumonies; also to Cleghorn, for the fact that, at Minorca, pleurisies are generally the chief among the vernal epidemics, as tertian intermittents are constantly among the autumnal; and, in addition, states, on the authority of Drs. Weeks, of Sussex, and Harrison, of Horncastle, as well as his own, that, with the decrease of intermittents in some parts of England and London, there has been a like decrease of pleurisies.[1]

Those who in Italy, England, France, and this country, deny or doubt the agency of malaria in the production of periodic fever, though not all, so far as I know, going the length of regarding pneumonia and other thoracic diseases as nothing more than peculiar forms of such fevers, cannot but believe in their close alliance with these, viewing, as they do, the latter as proceeding from the agency of much the same causes as occasion the former. "That a residence in marshy countries, says one of these writers, subjects to intermittent fevers, is an undeniable fact; and that they who live on broken, hilly districts, are liable to pulmonary diseases is also unquestionable. If locality explain the latter, it may equally explain the former, without recurrence to imaginary agencies. In the same county of Lincoln, in England, the inhabitants of the fens are

[1] Trans. of a Soc. for the Improvement of Med.-Chir. Knowledge, iii. 537–9.

sufferers from intermittent fevers; those of the wolds or hills, are obnoxious to catarrh, pleurisies, and phthisis. If an exchange be made of habitation in those two cases, there will be exchange of diseases. Why then demand miasm as a cause of fever, and refuse it as a cause of pulmonary disorders?"[1] As the writer regards autumnal fevers, from the simple intermittent to the malignant yellow as produced by heat, cold, humidity, atmospheric vicissitudes, and the like, and as he cannot attribute pulmonary inflammation to other agencies, it follows that, according to him, the same causes may give rise to both sets of diseases, which hence, cannot differ essentially in a pathological point of view.

Dr. Rush and his disciples may not always have said, in positive terms, that pneumonia, pleurisy, and other kindred affections, are really and substantially peculiar forms of periodic fever, and nothing more;—they may not have attributed them all to the same morbid agencies; but in upholding the doctrine of the unity of disease, and the applicableness of the same treatment—modified only in point of energy, and as regards the use of particular means, by the condition of the system at large, and the nature of the parts affected—they admitted the pathological identity of pneumonia with periodic fevers, considering them both as constitutional diseases, which assume different forms according to the nature of the local derangements by which they may be accompanied, but remain always fundamentally the same. Hence we find that eminent physician remarking: "There is but one exciting cause of fever, and that is stimulus. Heat, alternating with cold, marsh and human miasmata, contagion and poisons of all kinds, intemperance, passions of the mind, bruises, burns, and the like, all act by a stimulating power only, in producing fever. This proposition is of great application, inasmuch as it cuts the sinews of the division of diseases from their remote causes. Thus it establishes the sameness of a pleurisy, whether it be excited by heat succeeding cold, or by the contagion of the smallpox and measles, or by the miasmata of the yellow fever." "There is but one fever. However different the predisposing, remote, or exciting causes of fever may be, whether debility from obstruction or action, whether heat or cold, succeeding to each other, whether marsh or human miasmata, whether intemperance, a fright, or a fall, still, I repeat, there can be but one

[1] Bell on Miasm. Philadelphia Med. and Phys. Journ. ii. 316, N. S.

fever."[1] A little farther on, he makes several forms or states of fevers. Of these, the eighteenth is the *pulmonary*, which includes true and bastard pneumony, acute and chronic catarrh, etc.

Morton, Lauder, Sauvages, Alibert, Mongellaz, Chauffard, Comparetti, Gouzée, Daniell, Evans, Abloing, Brera, Matthéi, Bailly, and others, to whom I shall again have occasion to refer, describe a pneumonic form of intermittent fever, or periodic form of pulmonary inflammation, produced by the same cause as ordinary intermittents, and thereby, like the preceding writers, acknowledge the identity of both sets of phenomena represented in the compound.

Not very different were the views of the late Dr. Robert Jackson, of the English Army, whose writings on the yellow fever of the West Indies, and South Coast of Spain, as also his volumes on the remedial effects of cold affusion in febrile diseases and on contagious fevers have enjoyed, and continue to enjoy, a merited reputation. Fever, according to this distinguished author, is a form of changed or perverted organic action, "that is, a new form of life impressed on the minuter series of organic capillaries, varied in expression according to the varied structure of the series upon which the act is principally manifested." "The diseased act is liable to change, or to suffer transfer from one series to another, in various ways and manners, and at various points of time; and thus to exhibit within the limit of the total duration, a scene of fluctuation and uncertainty which is more or less embarrassing to the observer."[2] After stating that he considers endemic fever, whether it appears in the torrid or temperate zones of the earth, to be radically one disease, he remarks: "As the act, whether progressive or regressive, varies under contingencies, so it changes mode, or suffers transfer from one series to another at certain periods of its course, to such an extent and in such manner as to appear totally unlike itself. The modes of febrile action are, as now observed, numerous, and, as superficially varied, totally unlike one another. The author considers them as resting on a common base, consequently as one disease." So much for endemic fevers—the cause has the same base in all parts of the earth, modified by circumstances of locality in different districts of similar latitudes, so as to present considerable diversity of appearance in its visible operation. It is

[1] Outlines of the Phenomena of Fever, Works, iii. 9, 10.

[2] A Sketch of the History and Cure of Febrile Diseases, i. 10, 11, 15. Lond. 1820.

also modified by elevation, exposure, and the revolution of seasons, circumstances of subjects, &c. Farther on, Dr. Jackson says: "The outline of the history and cure of fever, which is given in the preceding pages, applies to fever as a disease of the organic system acting on a general base. Fevers, I am aware, rarely occur at any time, where some one part does not suffer more prominently than others in all stages of the course; but the term *general* is here affixed to that form of the disease where the predominance is fluctuating and contingent; *local*, where it is prominent at the commencement, and where it continues uniformly prominent throughout. The cause which produces inequality in the force and mode of the perverted movements, which are excited by the action of the cause of fever in the different organs of the system, is necessarily obscure. I do not pretend to explain it, and I only take leave to suggest, that it is apparently connected with the unequal condition of organic sensibility existing at the time the morbid cause is applied, or that it explodes into action; a condition constitutional or contingent, as depending upon the operation of general or contingent causes to which the movements of animal life are exposed."[1]

We have, then, according to this view, fevers, which are diseases of the whole system, without determinate and permanent local determinations; and fevers also of the whole system, but which, unlike the former, manifest their action more permanently in particular organs: among the latter, we have the forms of fever located in the inferior or abdominal cavity; the gastric, choleric, dysenteric. The forms of fever with local action in the superior or cranial cavity; the forms of fever with local disease in the organs of the thoracic cavity—the pneumonic, the cardiac, and catarrhal; and the forms of fever with external local action—the ophthalmic and ulcerative. All these forms are produced by the same causes, though some are more particularly noted to owe their origin to exhalations from the soil, more generally or more partially diffused; all are mere modifications of one and the same disease.

"The pneumonic," adds Dr. Jackson, "is an important, and, in some countries, a frequent form of the action of a febrile cause. It is more common, according to the laws of the annual revolution, in some seasons of the year than in others; and it is sometimes epidemic in seasons and places to which it does not seem to belong."[2]

[1] *Op. cit.* ii. 1, 2, &c. [2] *Ibid.* p. 83.

If anything were required to show that all those febrile diseases with local determinations were placed by Dr. Jackson on the same footing, pathologically and etiologically, with the non-local forms, it might be derived from the fact that, while including, as we have seen, dysentery and some other complaints among the former, he remarks, after stating that on plains near the sea-coast, muddy rivers, bayous, and other foul grounds, the product of the cause is usually remittent, sometimes intermittent: "The action of the morbid cause, instead of being what is commonly called febrile, is not unfrequently dysenteric; sometimes eruptive and ulcerative on dry, bare, rocky, and hilly positions near the sea-coast, or in positions where water flows with a rapid course."

Dr. Macculloch,[1] though furnishing no proof of the correctness of his opinion, and admitting that, of very much of what he states he has no personal knowledge, includes among the diseases produced by malaria, and which consequently he considers as mere modifications of the legitimate products of that cause, angina, asthma, œdema of the lungs, and *catarrh reaching to peripneumony.* Copland, in his article catarrh,[2] echoes Macculloch, so far, at least, as the latter disease is concerned, and, under another head, refers *catarrhal fever* to the same cause as intermittents, enlarged spleen, torpid states of the liver, and rheumatic attacks, *i. e.* to "miasms from decayed vegetable matter, aided by moisture, in temperate ranges of atmospheric heat."

An intelligent writer of our own country, the late Dr. Vaughan, of Wilmington (Delaware), in an Essay on the diseases of that State, alluding to those of winter and spring, remarks that a respite is then obtained from the attacks of ague, and a new order of diseases arises from sudden transitions of temperature. The general character of the prevailing complaint is peripneumonia notha, in the majority of cases; but too much stress, he thinks, is laid on the local affection, and too little on the general state of the system; and he farther maintains that, from the state of the pulse, the frequent sighing in respiration, the tendency of the fever to assume a tertian type, the early appearance of the hippocratic countenance, "it belonged to the genus of autumnal fevers, varied by the casualties of the atmosphere." "It may be said that this is too far strained, and that frost destroys marsh miasmata as a tropical plant. Granted:

[1] On Malaria, 442.

[2] Vol. i. 272.

when the ponds are eovered with iee, exhalation is overpowered; but if these fevers suffer a common fate with their eauses, why do intermittents exist or oecur in winter, and winter quartans, the most obstinate of the whole tribe? Why do valetudinarians suffer relapses in the frosts of January and February? And why are persons on a removal from a marshy to a high eountry attacked with the endemial fevers of the fens, if the remote eause be not dormant in the system, and exeited into aetion by a eoneurrenee of predisposing eauses? These faets, I presume, substantiate the position that our winter diseases are but varied forms of the autumnal fever."[1]

In an account of the diseases whieh oeeurred at Fort Gibson (Arkansas), during the last quarter of 1833, Dr. Forry states, on the authority of Dr. Pitcher, and Dr. Wharton, of the army, that most of the eases—pleurisies, eholera, rheumatism—partook of the intermittent eharacter, and adds: "The striet periodicity of these affeetions, and their subjeetion to the same remedies whieh are found to arrést the eourse of intermittent fever, imply a close allianee, if not a eommon origin."[2]

We might embraee, in this eategory, no less an authority than Broussais, and even the greater number of his more enthusiastic disciples; for, with them, all fevers, whether of a continued, remittent, or intermittent type, are nothing more than so many varieties of one and the same pathologieal condition; inflammatory irritation having its seat in one and the same tissue—the gastro-enteritic mueous membrane. This gastro-enteritis, with the various eomplieations that may supervene, are, they say, produeed by the aetion of eold alone, or, as is more frequently the case, eombined with humidity, or through the agency of atmospheric vieissitudes; or, as regards some forms of the disease, of malarial exhalations. But, whiehever of these may be the effieient agent—heat, eold, humidity, vieissitudes, or malaria—the effeet is the same. It is always gastro-enteritis, whether with or without the addition of inflammatory irritation in other organs or tissues. There is nothing speeific in the eause, and, as a natural eonsequenee, nothing speeifie in the effeet which that eause produees. Heat, eold, malaria, may go a different way about it, but the ehange they induee in the system is similar, and that ehange is effeeted in the gastro-enteritie mucous membrane. One or other of the pyrexiæ is the eonsequenee; the differenee be-

[1] New York Med. Repos. iv. 130.

[2] Climate of the U. S. 186.

tween them being due to a variety of concomitant circumstances, connected with external influences or personal peculiarities. Pneumonia, and other inflammations, parenchymatous, serous or mucous, differ in nothing from the other form of disease; they are, it is true, more frequently produced by cold and atmospheric vicissitudes than by malaria. But the effect, after all, is only inflammation; and, as the latter presents nothing specific in its character, whatever be the part it may show itself in, it follows that these diseases differ in nothing from the former mentioned, except as regards the parts inflamed; the difference being due to the mere circumstance that, at the approach of winter, or during the prevalence of atmospheric vicissitudes, the thoracic organs take on more readily the inflammatory action, while at other periods the gastro-enteritic apparatus is more frequently affected.

More recently, Dr. H. A. Ramsey, of Raysville, Geo., in an Essay on Pneumonia, published in the early part of 1851, has advanced much the same views, regarding the disease as the product of the same causes as occasion intermittents, and hence, as identical with these.[1] And at a still later period, Dr. A. P. Merrill, Professor of Materia Medica and Therapeutics, in the Medical School of Memphis, Tenn., in an Essay on Pneumonia, originally inserted in the number for July, 1851, of the *New Orleans Med. and Surg. Journ.*, but more recently issued in pamphlet form, conjointly with two other essays, has adopted a similar theory, relative to the connection in question. Asserting it more positively, and with much greater distinctness than has been done by the generality of modern writers, and regarding it as fully sanctioned by the results of a long experience, acquired in the South, he lays down as an indisputable fact, that pneumonia, though sometimes appearing as an idiopathic affection, is most generally—whether it prevail sporadically or as an epidemic—really and substantially nothing more than a peculiar form of remittent and intermittent fever, in which the lungs, from sudden transitions of temperature, are made to bear the burden of local disease.

Other authorities, equally respectable, both at home and abroad,[2]

[1] A Practical Essay upon the Symptomatology, Etiology, Vital Statistics, and Treatment of Pneumonia.—*Charleston Medical Journal and Review*, vi. 1, etc.

[2] Baronius, Pleuro-pneumonia, Ann. 1633. Bovillat, Mém. sur les pleuro-pneumonies épidemiques, 556. Good, Study of Med. ii. 424. Hugh Williamson, Med. Register, iii. 453. Boott, Life of Armstrong, ii. 41, 290–1, 416. Yates, an Essay on

might easily be found, to swell the list of the advocates of the views in question; but the preceding will amply suffice to show that these, whether having reference to a few exceptional cases of pneumonic inflammation, or to the disease as it appears always and everywhere, and as it presents its legitimate characters, have long enlisted the attention of medical inquirers, and continue to enumerate warm supporters.[1]

When we come to inquire into the grounds upon which the opinion of this close alliance or identity, both as regards the pathology or etiology of those two classes of diseases is predicated, we find that, with the exception of Sydenham, according to whom the difference of one epidemic disease from another depended on a variety of atmospheric constitutions, and who contented himself with describing the symptoms, progression, &c. of the complaints he observed and

the Bilious Fever prevailing in the State of New York. Albany, 1813, p. 27. Med. and Phil. Register, iii. 488. Sarcone, Histoire Raisonnée des Maladies Observées à Naples pendant le cours entier de l'Année 1764, i. 124–202.

[1] A writer in one of the Western Medical Journals (*Ohio Medical and Surgical Journal*, i. 508) remarks, that Malaria, besides giving rise to *all varieties* and *species* of acute febrile disorders—from the highest sthenic to the lowest asthenic grade, induces many obscure and anomalous forms of disease, and frequently imparts new features and tendencies to every disorder that may come within the sphere of its influence. That it makes its impressions primarily on the *cerebra spinal system*, can hardly, as he thinks, admit of a doubt. The morbid impression there made may be transmitted to the extremity of the nerves taking their origin in that system, and there develop its evidences, in the form of some functional or organic derangement. "The viscera of the chest, abdomen, and pelvis, and the fibrous cellular tissues, may in this way become the seat of disease, from reflected malarious impressions. Such cases are common. Many affections of the heart, stomach, intestines, and liver, and in females uterine disorders, are of this character. They have been imperfectly recognized and described by authors as "Irregular and masked Intermittent," "Complications," &c. This exciting cause may restrict its action solely to the nervous system, and has been known to originate or complicate with every species embraced in Cullen's class Neuroses, from *Apoplexy* down to *Hysteria*. In other instances, it may take a wider range, and show its effects under the forms of irritation, inflammation, and I might properly add, every disorder to which the human family is subject." In all cases of malignant erysipelas that have come within the experience of this writer, "it has participated largely, either as an exciting or modifying cause." To the Western physician, it is added, under whose observation malaria is daily producing such effects, their diagnosis is often a source of perplexity; while to the members of the medical profession in the Eastern States, "who know nothing of such diseases, except from books and lectures, it is a perfect stumbling-block."

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troubled himself very little about their pathological or phenomenal relationship; and of Dr. Rush, with whom the idea of the alliance in question was the natural offspring of his doctrine of the unity of diseases; most of the writers we have cited, from Cleghorn downward, dwell on the circumstance, that the two diseases—periodic fever and pneumonic inflammation—are found to prevail simultaneously in the same localities, or to succeed to each other; that while pulmonary inflammation often occurs in summer and autumn, when fevers are rife, so the latter—even intermittents—appear also in winter and spring, when the former usually prevails extensively.

With Dr. Ramsey, whose observations were principally made during an epidemic of pneumonia which swept over Lincoln County, Georgia, in 1845, the proof lies principally in this: that families who have been formerly healthy at other points, become affected by intermittent, febrile, and pneumonic diseases, by moving to a malarious location, and subsequently healthy by removing from it; that on that remarkable occasion, when out of a population of seven thousand, three hundred died of the disease, and when out of one hundred and seventy cases he attended it was his good fortune "not to have sustained the loss of a single one," most of the cases occurred along the course of the streams, and at those points which rarely escape chill and fever; that when it occurred elsewhere it could be accounted for by the fluctuations of the seasons, manure-piles, swamps, &c., which then and now exist throughout the country to a greater or less extent, "carrying with their effluvia, through the medium of the winds, the emanations of disease (pneumonia of course among these) and the seeds of death," and that, in ordinary times, "those who make most manure, and have their lots closest to their houses, have most cases of pneumonia." Pp. 12–16.

Dr. Merrill, who has recently taken a leading part in the defence of the opinion in question, and who, on more accounts than one, deserves to be listened to with respect, on this or any other subject connected with the diseases of our Southern States, rather boldly assumes the point, than attempts to prove it, and remarks: "These fevers occupy the attention of the physician, in some of their various shapes, at all seasons of the year, both in town and country; and they have been characterized, at various times and in various places, by a great multiplicity of names." Again: "This protean character of our fevers arises in part from the season of the year, and the localities in which they occur; but mainly from the organs

of the body which become involved in the diseased action, the predominance of inflammation or congestion, and the character of their periodicity. In the spring we are apt to find these diseases assuming names which have reference more particularly to this periodicity and general pathology. As summer comes on, the greater implication of the hepatic organs changes the name, or adds an epithet to designate a prominent symptom. In autumn, the chylopoietic viscera become more strikingly involved in the diseased action, and this again is indicated by an ever-changing nomenclature. But when winter approaches, and the subjects are exposed to sudden transitions of temperature, the thoracic viscera are called upon to bear the burden of local disease, and then it is that the names pleurisy, pneumonia, pneumonia typhoides, pneumonia biliosa, pleuro-pneumonia, bilious pleurisy, lung fever, etc., become familiar."

After a pointed allusion to writers of books on the practice of medicine, and teachers who "find a complicated system of nosology a very convenient loop upon which to hang their learned disquisitions;" as well as to physicians in practice, who "derive advantage from an exercise of ingenuity and tact, in suiting the names of prevailing diseases to the phases of popular prejudice, and in explaining their want of success, without implicating the infallibility of their skill;" and also to the erudition displayed in arranging the nosology of "Southern fever into forty or fifty different varieties, deriving a technical name for each of them, from the classical lore of the schools, and from the more vulgar vernacular of modern tongues," etc., Dr. M. adds: "Let us talk as learnedly, and refine, discriminate, and vary our nomenclature as we may, to suit the fashion of the times, when we come to deal with plain facts, as they are presented to us in practice, this whole class of diseases to which I have here alluded will be found to take its appropriate place under the plain designation—*periodic fever*. We know very little of the causes which produce it; but we meet with it every month of the year, and generally it varies in appearance and symptoms only as it varies in degree of violence, and as the different organs of the body become more or less implicated. Now it so happens, and we need not attempt here to give the reasons why, that the stomach and brain become more involved in the diseased action in summer, and the lungs and the other thoracic contents, in winter." "These local affections, whichever may for the time predominate, do not, in any material respect, change the character of the constitutional dis-

ease, it is a periodic fever still, and requires the anti-periodic treatment." "In all its forms and modifications, we can never lose sight of the important fact that the disease is one of periodicity and self-limitation. Its uniform paroxysmal character points to the use of one great remedy, upon which, in all cases, whenever (wherever?) the local lesion may appear, we must place our main dependence for relief," care being taken, however, to remove those lesions by appropriate means.

All this is plain enough, and leaves no doubt as to the import of the views under examination, of the principal grounds on which they are founded, and of the mode of practice to which they are intended to lead. Pneumonia must, henceforward, be classed among the various diseases which, from a peculiar phenomenon they exhibit, and the peculiar treatment they call for, take their appropriate place under the plain designation of *periodic fever*. Pathologists, opening their eyes to the light once more shed upon the subject, must cease to consider pneumonia, except in a *few* cases, as an idiopathic disease separate from and independent of all others. They must learn to view it as only a *form* of another complaint; in other words, as a periodic fever in disguise. Its occurrence at the same time, and in the same place with periodic fevers; its succeeding to or preceding these; its presenting periods of exacerbation and remission, and its being benefited, when these fluctuations occur, by remedies, quinia particularly, which exercise an anti-periodic power, establish beyond the possibility of doubt the important fact of the close alliance between, if not common origin of, those diseases. All the diseases so classed, pneumonia, pleurisy, pneumonia typhoides, pneumonia biliosa, as indeed, hepatitis, phrenitis, gastritis, are products of the same causes that occasion remittent, intermittent, yellow fevers, plague, Asiatic cholera, and typhoid fever—the difference depending on temperature, atmospheric vicissitudes, and the like.[1]

[1] In former publications,[1] Dr. Merrill has expressed the opinion that yellow fever differs in nothing but degree of violence from the common autumnal remittent, and arises from precisely the same causes. I need scarcely remark that many others, in this country and elsewhere, have entertained, and continue to entertain, similar views on the subject. For this reason, and because, while discarding all such opinions relative to the pathological and etiological unity of these two forms of fevers, I regard yellow fever, as also common autumnal fevers, as the products of aerial poisons—the

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[1] "Yellow Fever of Natchez," in 1823, *Philadelphia Med. and Phys. Journ.* ix. 235; "Yellow Fever of Natchez," in 1825, *N. A. Med and Surg. Journ.* ii. 217.

Autumnal fever—it is argued—being met with every month of the year, the cause, whatever it may be, must exist all the year round, uninfluenced, in regard to its creation or disappearance, by changes of season, atmospheric phenomena, etc. Like every other form of fever, pneumonia must be viewed as a general disease of the whole system, but evincing a strong determination of diseased action to some particular organ—in the instance before us, to the lungs—the difference depending on season, weather, and other kindred circumstances. These local affections, whichever may for the time predominate, do not, in any material respect, change the character of the constitutional disease—it is a periodical disease still, and requires the anti-periodic treatment. The pneumonia of all writers is, strictly speaking, only the pneumonic form of periodic fever; and while we are devoting our best skill and energy to relieve the local affection, we must not lose sight of the fact that we are dealing with a constitutional complaint—a periodic fever. Though it cannot be denied that pneumonia sometimes appears among us, as an idiopathic affection, we are justified in concluding that the thoracic inflammations, which show themselves in this country generally, sometimes sporadically, and frequently with epidemic violence, are really and substantially nothing more than a peculiar form of intermittent and remittent fever. In a word, pe-

results of local infection—of kindred though not identical nature, I shall, throughout these pages, treat of them all under the generic name of malarial diseases. They all belong to that family of zymotic diseases (those due to morbific ferments), which, unlike others of the same class, which are due to contagious viruses, arise from local sources of infection; in other words, in which the morbific emanations in connection with which they arise, act not only as predisposing, but as exciting causes also. The admission that these diseases belong to the one class of zymotics and arise from ferments, by no means necessarily carries along with it the idea of pathological identity.

For similar reasons I may occasionally refer to typhoid fever in connection with the cause of the former, for that disease would appear to arise, sometimes at least, from, or to be associated in its production or propagation with, morbid agencies somewhat allied to those from which autumnal fevers generally spring. Besides it is viewed, with what degree of propriety I shall not stop here to inquire, by some of our Southern physicians, to say nothing of some few in the North, and a small set in England, as only a peculiar form—the continued—of common autumnal fevers, and not as a special and specific disease.

Finally, I shall, for tho same reason, refer to the oriental plague as a malarial disease; for it is not unusually acknowledged, by high authorities too, to arise or to be greatly under the influences of exhalations of a malarial character; and is, besides, regarded by some writers of this country more particularly as another form of common remittent autumnal fevers.

riodic fever is a protean disease, which sometimes, owing to sundry adventitious circumstances, attacks one set of organs, sometimes another. When it exercises its deleterious influence on the pulmonary organs, it gives rise to their inflammation or congestion, and occasions the disease we denominate pneumonia, pleurisy, etc. But, whatever be the part affected by it, it is always the same constitutional disease, and invariably calls for the same general treatment. Medical writers and medical teachers may amuse themselves in classifying diseases according to their supposed differences in regard to seat, phenomena, and nature; they may exercise their ingenuity and tact in suiting the names of prevailing diseases to the phases of popular prejudice; or display vast erudition in discriminating the different varieties of southern fevers, and arranging their nosology; they may prate about these matters as much as they please, but, by so doing, they only afford the proof that they have not carefully imbued themselves with the principles of the Baconian philosophy, and that their whole system, of the pathology, practice, etiology and physiology of fevers and febrile affection is erroneous —founded on preconceived notions, and not upon facts and inductions. For it is now proved, to the satisfaction of a few observers, more sharp-sighted than the very large majority of their brethren in both hemispheres, that this whole class of diseases takes its appropriate place under the plain designation, *periodic fever;* that all the varieties of that class, which, for certain reasons, well known to the reader, have been held, heretofore, as distinct from each other— typhus, typhoid, intermittent, remittent, yellow, etc.—are really and substantially one and the same disease, and that in the same category must now be placed pneumonia, pleurisy, etc.

Such are the views entertained to some extent in former days, and revived on a rather more extensive scale in recent times, relatively to the close affinity or the identity—pathological and etiological—of two classes of diseases which the profession generally, both here and elsewhere, have found ample reason to regard as totally distinct from, and independent of each other. Examined in whatever point of view we please, analyzed as closely and minutely as it is in our power to do, the whole argument in favour of those views—setting aside what flows from the peculiar theoretical notions of some of their advocates relative to the unity or relationship of all diseases—and stripping it of all the *hors d'œuvres*, and hypothetical assumptions, and doubtful assertions by which it is

accompanied, and to a great extent disfigured, the opinion in question resolves itself always into a very narrow compass, and is found to rest on a few points already referred to. It may be stated thus: the two classes of diseases, pneumonia and autumnal or periodic fevers, prevail either simultaneously or consecutively in the same locality—conclusion: they must be due to the operation of the same causes, and therefore are identical in nature. The periodic element is more or less strongly marked in the various grades of autumnal fevers; the same element is sometimes detected in pneumonia—conclusion: the latter disease—to say nothing of many others of the same class—whether it shows itself sporadically or epidemically, is really and substantially nothing more than a peculiar form of remittent and intermittent fever. Pneumonia sometimes yields to the action of the same remedies which are found to check the course of autumnal or periodic fevers—conclusion: the similarity of results obtained from the same treatment in both classes of diseases, implies a close alliance between them, or, indeed, a common origin and pathological identity. Pneumonia must be viewed as a general disease of the whole system, but evincing a determination of morbid action to the lungs; the same may be said of autumnal fevers, with the exception that the local determination takes place in some other part—conclusion: both these classes of diseases being constitutional, and the difference of the local effects depending on fortuitous and secondary circumstances, they are produced by the same causes, and are identical in nature.

It is evident, from what precedes, that the modern advocates of the theory, or rather hypothesis in question, for it scarcely deserves the name of theory, may fortify their cause by an appeal to the imposing phalanx of authorities above referred to. No one will deny that among these several are found who are entitled, for many reasons, to the unbounded regard of the profession at large, and are destined to occupy an honourable position in the annals of our science. Neither can we doubt that others among them—perhaps a large number—deserve to be treated with courtesy, manifesting, as they generally do, practical good sense, a commendable amount of professional attainments, and no inconsiderable share of ingenuity and smartness. But however numerous the advocates of the hypothesis undoubtedly are, and whatever may be the degree of respect to which their opinions on this and other subjects is entitled, it need scarcely be observed that, in what has been adduced

in its support by them, we cannot discover sufficient reason to acquiesce in its correctness; while the circumstances under which some of the writers quoted were placed, and the opportunity for accurate observation and close and careful induction, and the degree of professional scholarship possessed by others, were not such as to render their verdict on pathological and etiological points beyond the reach of criticism, and to silence opposition on the part of subsequent inquirers. Some, and perhaps the most distinguished, flourished at a time when pathological and etiological knowledge was at too low an ebb to induce us to look to them for the settlement of questions of the kind. Others, though living at a period less remote from our own, do not appear to have sufficiently qualified themselves by diligent inquiry, and extensive and close study, to justify their pretensions to enlighten the medical world on subjects of this nature; while others, again, who recently have thought fit to revive the hypothesis, have evidently taken but a one-sided and contracted view of the matter, and allowed themselves sometimes to be swayed in their inferences by favourite and fanciful hobbies.

Under such circumstances the subject might, and perhaps ought to have been allowed to drop. But the importance of the question thus raised; the stir it has occasioned among the physicians of some sections of this country, and the fair professional standing of some of its modern supporters, have induced me to investigate it with all the care and attention of which I am capable. The result of this investigation has been unfavourable; and so far from leading me to lend a willing ear to the connection under consideration, has only tended to confirm me in the opinion I have always held on the subject. It has done more; for I have thereby been induced to adopt conclusions diametrically adverse to those of which I have presented an outline. Now, more than ever, I regard these as faulty in a scientific point of view, and leading to pathological deductions not only erroneous and at variance with long admitted principles, but occasionally glaringly illogical. I hold them to be in direct opposition to facts which are, or ought to be familiar to every observant and well-read physician. I hold them to evince a total oversight of all established opinions respecting the causes of the several diseases thus placed in juxtaposition. I hold also that, however plausible these conclusions may seem to be, they have not been supported, as far as I can perceive, by a single fact or argument

calculated, when properly sifted, to satisfy a careful inquirer, and to which a ready answer may not be found. I hold all this; and see, with regret, that the whole subject has been presented in a manner evincing a decided disposition to set at naught, as worthless, the observations and inferences of all preceding and contemporary writers who entertain adverse views, and not unfrequently in a trenchant and contemptuous tone, from which none, not even those whom the medical world has never ceased to treat with courtesy, and view in the light of standard authorities, are allowed to escape. For these reasons, and considering also that these opinions are likely to lead, if adopted and acted upon, especially by inexperienced physicians, to a hazardous practice; bearing in mind, besides, that, from the influential position which some of their advocates occupy, there is reason to fear they may continue to make converts, not only among medical students, but among that class of professional readers—unfortunately too large among us—who are easily seduced by real or supposed novelties, and fanciful or whimsical notions; and convinced, in addition, of the necessity of ascertaining, without loss of time, the value of dogmas bearing directly or indirectly on practical points, when presented and enforced by men of note and reputation, I propose to devote this volume to a survey of the facts and arguments that may be adduced, and thus to test, in its various details, the hypothesis to which attention has been called, and see how far it is deserving of our adoption.

The reader will easily perceive that, before the advocates of the identity of the two diseases can successfully sustain the position they have assumed, it will be necessary for them to show that autumnal fevers and pneumonia are produced by the same causes; that they prevail in the same places, and during the same seasons of the year; that their existence and diffusion are promoted by the same agencies; that they are arrested by similar means; that they exercise their effects on the same classes of individuals; that they present similar or kindred symptoms; that they affect the same organs, and produce the same or analogous changes in the fluids and solids; that they are governed by the same laws; and that they present other points of approximation invariably found to be possessed by diseases between which there exists the close connection claimed in the instance before us. Unless they can succeed in attaining these objects, their opinion must fall, and the independence of those diseases be admitted. It becomes necessary, therefore,

to take up each of those subjects separately, and to ascertain how far they may be appealed to in respect to the question at issue.

As we have seen, it is admitted that pneumonia is sometimes an idiopathic affection, caused, of course, in such cases, by atmospheric vicissitudes, or other kindred morbid influences, and unconnected with any complaint except ordinary phlegmasiæ. But, while admitting thus much, it is contended that, in the majority of instances, the disease must be referred to a different origin, and viewed as simply symptomatic; that it is a mere form of another complaint, and would not, it may be presumed, exist if the causes of the latter, whatever they may be, did not exercise their baneful influence. It is suggested that the combination of phenomena, which impressed the common observer with the idea that he beheld a special and independent complaint, was fallacious—really nothing more than a cloak, serving to disguise a disease which, in its natural state, assumes a different dress; and that, while with infantile simplicity we fancied we had before us a disease of well-known pathological character, it turns out that we were dealing with one of a totally different kind, in which the affection of the lungs constituted an accidental and unessential element.

All this may prove acceptable in certain quarters; it may be regarded as plausible by some, or even as well founded and perfectly unanswerable. But, so far as I am concerned, I have no hesitation in stating, as the result of my personal observations, aided by extensive inquiry and close reflection, that pneumonia is not *sometimes* but *always* an idiopathic disease, whether it occurs sporadically or epidemically; whether in the South, in the North, in the East, or in the West; whether in fever districts, or in fever seasons, or in places or at periods of the year free from periodical fevers; and that it is due to causes perfectly distinct from those to which such fevers owe their origin. I believe this; and believe, besides, that even were pneumonia produced at times by the legitimate causes of fevers, such cases would not be any more symptomatic than those that are due to the ordinary causes of the disease; for these do not act directly and primarily on the lungs any more than the others would do, supposing them capable of giving rise to the effect in question. And as in either circumstance the primary impression is received by some other part of the system, and thence reflected on the pulmonary organs, the two sorts of cases must be placed on the same footing, and be all primary or all secondary. Of

course we must exelude from the cases thus referred to, those pro dueed by traumatic or mechanical injuries, and agencies acting directly on the lungs, for these might, strictly speaking, be called symptomatie. It may, perhaps, also be proper to exclude some of those in which symptoms of pneumonia show themselves during the progress of other diseases, or after extensive surgieal operations or wounds. I say in some, for while in these the inflammation of the lungs is secondary, and merely the effect of sympathetic irritations, whieh may and do arise in a variety of dissimilar eomplaints, and are somewhat influenced by the nature of the eause giving rise to them; in others, the pneumonia may properly be ealled idiopathie, inasmuch as it is independent for its eausation of the agency giving rise to the prior eomplaint, being produeed by the same eauses whieh would have given rise to it had no sueh disease, operation, or wound preeeded. But, whatever be the mode of origin of sueh eases, many of whieh properly belong to a eategory to whieh attention will be called by and by, it may safely be maintained that pneumonia, whenever it shows itself *ab initio*, is idiopathie, that it is always produced by its appropriate eauses, and, what is more to our present purpose, that it is independent for its origin of those morbid ageneies to whieh autumnal fevers are due; that it is not less independent of sueh fevers in a pathologieal point of view, and that it eannot, therefore, be lowered to the rank of a peeuliar *form* of that elass of diseases.

1. *Pneumonia common where fevers seldom or never are seen.*—It is scareely neeessary to remark that inflammatory affeetions of the substanee, and lining, and covering membranes of the lungs, prevail very extensively in plaees where remittent, intermittent, and other fevers of kindred nature are not observed, and during seasons of the year when, if the ordinary causes of fever had at any time exereised their influence, they have been effeetually or temporarily eheeked. With us, pneumonia prevails, during eertain periods of the year, in loealities where autumnal fevers, espeeially intermittents, rarely if ever originate. In Philadelphia—and the same may be said of other large eities—sueh a thing as a case of the latter disease is seldom encountered. When the disease does present itself, it is usually seen among individuals who have taken it elsewhere. On the other hand, in our extreme suburbs and in the surrounding open country, where remittents and intermittents are sometimes quite rife, and always, except in winter, more or less notieed, pneu-

monia is not much more common than we find it to be in the city proper; not more so, at least, than can be accounted for by the greater exposure of the inhabitants to atmospheric vicissitudes and other known causes of the disease. Within the limits of the bills of mortality, including the city proper, where periodic fevers are scarcely ever seen, and a few only of the fever districts, the proportion of deaths from inflammation of the lungs and bronchiæ, in thirteen years, 1836–1848, amounted to 1 in 11.7.[1] Again, in the cities of our Middle States, the yellow fever, which, as we have seen, approximates to, without however being identical with, ordinary autumnal fevers, shows itself only in certain confined and peculiarly circumstanced localities, especially along and near the wharves. Now if there are facts to show that pneumonia is more common in such localities, during sickly seasons or at any other time, than in other places where the fever never reaches, unless imported, an experience of upwards of thirty years has not enabled me to discover them, and I am sure no one has been more fortunate than myself on that score. Most American medical readers know, or if they do not, a perusal, among others, of Dr. Holmes's excellent *Dissertation on Intermittent Fevers* will teach them, that this disease is of rare occurrence in New England, except in certain circumscribed localities to be noticed by and by, and that it has disappeared in places where some years ago it prevailed more or less extensively. It may be remarked, also, that in other localities of this country, and in various cities and rural districts of France, England, Tuscany, Lucca, and other parts of Europe, to which I shall have occasion to call attention as I proceed, intermittents, which before had spread extensively, have so effectually been chased away, by the judicious application of hygienic means, as to be now but rarely encountered. But while such has been the case with regard to malarial fevers, we are perfectly safe in affirming that pneumonias have not disappeared together with the former from any of those spots of New England, which have so much improved in point of salubrity; that the disease prevails over the whole region now as it did formerly, and that hence there are few physicians better acquainted with its phenomena than our brethren of the Eastern States. Among our troops in the military posts on the coast of New England, the ratio treated per thousand of mean strength,

[1] Trans. of Coll. of Phys. ii. 375.

amounts, according to Forry, to 41.[1] On a mean strength of 3,138, there were 233 cases of catarrh and influenza, 22 of pneumonia, and 26 of pleurisy.[2]

Nor is it less certain that the same remarks are applicable to the localities in Europe above referred to; for there pneumonias continue to prevail as extensively and fatally as when their insalubrity from malarious influences was most noted. Take Paris and London as examples. According to Dr. Lombard, pneumonia constitutes, in the former, one-fourteenth part of the diseases of adult subjects, while in children the proportion varies from one-fourth to one-fifth.[3] The correctness of this estimate is confirmed, so far as regards adults, by the results obtained by Grisolle, who, on comparing, during three years, the frequency of that disease, relatively to that of all other internal complaints, in the hospitals of that city, found that the former constituted the fourteenth, fifteenth, or seventeenth part of acute and chronic complaints. In asylums for old people, the average proportion of pneumonias appears to vary from one-sixth to one-seventh.[4] To this it may be added, as corroborative of the extreme frequency of pneumonia in Paris, that the average mortality in the ten years, 1837–1848, from pulmonary catarrh, amounted to 2,222, and that from pneumonia to 2,637.[5] I am aware that a contrary statement has been made in regard to London and other parts of England. Dr. Wells, as we have seen, insists on the decrease of thoracic inflammation in that city since the diminished prevalence of malarial fevers, and appeals to other physicians for the occurrence of a similar result elsewhere. A Mr. Weeks, of Sussex, informed him that genuine pleurisies were not seen there now as formerly. Dr. Wells farther refers to Dr. Harrison, of Horncastle, who mentions incidentally, in different parts of *An Essay on the Rot in Sheep*, that both agues and acute inflammation of the lungs are less common in Leicestershire than they used to be. "According to Sydenham," Dr. Wells continues, "no disease was in his time more frequent than pleurisy. At present, a physician in considerable practice here may pass several years without seeing a single legitimate instance of it." Dr. Wells, it is true, finds that Dr. Willan, in his *Report on the Diseases in London, from* 1796 *to* 1800, makes frequent mention of thoracic inflammations; but he believes

[1] Forry; Climate of U. S. 238.
[2] *Ibid.* 242.
[3] Archives Gén. de Médecine, xxv. 68.
[4] Traité de la Pneumonie, 127.
[5] Trébuchet; Ann. d'Hygiène, xlvi. 20.

that that eminent physician must have called diseases by the name of pleurisy which differed considerably, in regard to the mode of treatment they required, from the same complaint referred to by Sydenham; and closes with the remark that the instances of acute inflammation of the contents of the chest which fell under his observation, in London, were almost solely peripneumonias, in which copious bleeding was less clearly indicated, and was attended with less benefit than commonly happens in pleurisies; and one-half, perhaps, of these instances, which were not numerous, supervened to acute rheumatism.[1]

Without stopping to examine how far the reasons assigned by Dr. Wells for doubting the frequency of thoracic inflammation in London are correct, and without denying the possibility of the decrease of pleurisy in Sussex or Horncastle, I may remark that the statements of eminent writers and statisticians do not bear that physician out in his conclusions. According to Dr. Farr, who has devoted much attention to the subject,[2] while in all England the mortality from pneumonia amounted in 1838 to about one twenty-fifth part of the whole number of deaths, and to about one-twentieth of those from internal diseases, in London the proportion amounted to about one-fifteenth of the whole deaths, and one-thirteenth of those from internal complaints. In London, the average deaths from 1820 to 1831, inclusive, occasioned by inflammation of the lungs and pleurisy, amounted to 2,077.[3] In 1839, while the deaths from those diseases in England and Wales amounted to one in 18.01 of the mortality from all causes excepting external violence, the loss in London was one in one hundred and twenty-nine, being 3,687 from pneumonia, and sixty-five from pleurisy. In 1840, the proportion did not differ very materially from this. Sir Gilbert Blane states that, during ten years' service at St. Thomas's Hospital, he attended upwards of 320 cases of pulmonary inflammation, and 192 of intermittent fever. In private practice, during another period of ten years, 1795 to 1806, he attended, out of 3,160 cases of various diseases, 145 of pulmonic inflammation, and only twenty-five of intermittent.[4]

Other facts lead to the same conclusions. If we turn to Nova

[1] Transactions of a Society for the Diffusion of Med.-Ch. Knowledge, iii. 539–541.

[2] W. Farr, First Annual Report of the Registrar-General of Births, Marriages, &c. London, 1839, p. 168.

[3] Marshall, Mortality of London, last table.

[4] Select Dissertations, i. 205–247.

Scotia, Malta, Bermuda, and Gibraltar, and examine how matters stand there in relation to the question before us, we shall find that malarial fevers are rarely encountered, while both residents and visitors suffer extensively from pneumonia. In Gibraltar, Dr. Hennen[1] states that malarial fevers seldom show themselves, while the mortality from pulmonary inflammation is very large.[2] The tables furnished by Major Tulloch, show that, in an aggregate strength of 60,269, the admissions among the troops for quotidian, tertian, and remittent fevers, amounted in nineteen years, 1819–1836 (exclusive of cases occurring during the yellow fever epidemic of 1828), to 616, and for yellow fever to four. At the same time, the number of cases of inflammation of the lungs reached 2,515, and of pleurisy twenty-eight, or 2,543 ; giving an average of 42.3 per 1,000.[3] In Malta, with a strength of 40,826, the admissions for periodic fevers (intermittents and remittents) amounted to 695, or seventeen per 1,000 ; those for inflammation of the lungs to 1,370 ; and for pleurisy, to twenty-one—total, 1,391 ; or thirty per 1,000 ;[4] the mortality being one-fifteenth of the whole.[5] In Bermuda, in an aggregate strength of 11,721, those same fevers—intermittent and remittent—amounted to forty-six, and inflammation of the lungs and pleurisy to 441, or seventeen per 1,000. In Nova Scotia and New Brunswick, malarial fevers figure in the tables for 52, while inflammation of the lungs presents an aggregate of 1,505, to which must be added seventy-two cases of pleurisy, which, the strength being 44,120, gives us a proportion of thirty-five per 1,000. It is proper to state that I have excluded, in this comparison, the common con-

[1] Topography of the Mediterranean, 490.

[2] *Ibid.* 498.

[3] Mortality of the British Army, pp. 7–11.

[4] At page 64, Major Tullock states that, from 1830 to 1836, the aggregate strength at Gibraltar being 22,868, 655 cases of pneumonia and pleurisy were admitted, or twenty-nine per 1,000. At Malta, the strength being 15,031, the cases amounted to 456, or 30.3 per 1,000. At page 17, we have the following table, which exhibits at a glance those proportions in 19 years :—

Pneumonia.	Aggregate strength in 20 years.	Admission P. and Pl.	Per 1,000
Gibraltar,	60,269	2,543	42
Malta,	40,826	1,391	34
Ionia Island,	70,293	2,272	32
Bermuda,	11,721	441	37
Nova Scotia and New Brunswick,	44,120	1,577	35

[5] Mortality of the British Army, pp. 22, 23.

tinued fever mentioned in the reports. Its origin from the malaria of common autumnal fever is doubtful, and being found to prevail in the most diversified climates, it may be viewed as the typhoid fever of most modern writers. At Constantine, in Africa, paludal fevers are not common, and, according to Dr. Antonini, physician in chief of the French army of occupation, when they appeared among the soldiers, they had most generally been taken elsewhere. Thoracic inflammations, on the contrary, are very frequently noticed[1] in that vicinity. Pneumonias are very common in Chili, at all seasons of the year, though more particularly during the spring months. On the other hand, intermittents and other forms of malarial fevers are there rarely encountered.[2]

The city of Turin, in Italy, is exempt from those fevers; but pneumonia is of common occurrence.[3] Similar observations have been made at Genoa.[4] Sweden, except in some few of its southern and alluvial districts is, like several other northern countries, free from malarial fevers. But so frequently is pneumonia encountered there that, by Hoffman, it was viewed in the light of an endemic.[5]

2. *Pneumonia not necessarily prevalent where fevers are common.*— If now we reverse the proposition, and inquire into the frequency of pneumonia in malarious and fever localities—a frequency which we might reasonably expect to occur, were the causes of the diseases and their pathology the same—we shall find little reason to admit the correctness of the hypothesis under examination. Hippocrates, it is true, may be, and indeed has been, appealed to in support of the fact that pneumonia is of common occurrence in paludal countries; and there is no doubt that if his testimony can avail in the settlement of this matter, and be considered conclusive, the advocates of the opinion in question may congratulate themselves, for the language of the Coan physician is explicit: "And I wish to give an account of the other kinds of waters, namely, of such as are wholesome, and such as are unwholesome, and what bad and what good effects may be derived from water, for water contributes much towards health. Such waters, then,

[1] Boudin, Georgia Med. 80.
[2] Lafargue; Bulletin de l'Académia de Médecine, xvii. 178–203.
[3] Mém. de l'Acad. de Méd. xiv. 230–1.
[4] Ann. d'Hyg. xxx. 58; xxxv. 5; xxxvi. 304.
[5] Williams; Cyclop. of Pract. Med. iii. 408.

as are marshy, stagnant, and belong to lakes, are necessarily hot in summer, thick, and have a strong smell, since they have no current; but being constantly supplied by rain-water, and the sun heating them, they necessarily want their proper colour, are unwholesome, and form bile; in winter they become congealed, cold, and muddy with the snow and ice, so that they are most apt to engender phlegm and bring on hoarseness; those who drink them have large and obstructed spleens; their bellies are hard, emaciated, and hot; and their shoulders, collar-bones, and faces are emaciated." "This disease is habitual to them both in summer and in winter; and, in addition, they are very subject to dropsies of a most fatal character; and, in summer, dysenteries, diarrhœas, and protracted quartans frequently seize them; and these diseases, when prolonged, dispose such constitutions to dropsies, and thus prove fatal. These are the diseases which attack them in summer; but, in winter, younger persons are liable to pneumonic and maniacal affections; and older persons to ardent fevers from hardness of the belly."[1]

In our days, the statement has received the sanction of no less an authority than the great Joseph Frank, from whom we learn that, in the course of an extensive practice, he never saw so large a number and so great a variety of pneumonias as among the peasant inhabitants of the rice fields of the canton of Tessino, where paludal fevers are endemic.[2] But facts in abundance may be collected to show that though under some circumstances pneumonia may prevail frequently, or even extensively, in localities subject to malarial fevers—as the environs of Strasburg, Pavia, Padua, Rome, Naples, the vicinity of Vesuvius, as well as in many parts of this country will attest—it frequently happens that in such localities, even where the paludal cause exercises its baneful effects with great energy, inflammation of the lungs is comparatively rare, and in some scarcely encountered. Indeed, there are not wanting reasons to justify the opinion that, on the point in question, the father of medicine must be regarded as an unsafe guide, and that his statement should be received with great caution. For, though in the marshy districts of Greece, inflammatory affections of the lungs may, for what we know, have been frequently seen by him—a fact which, as we shall see, recent observations do not tend to confirm—the

[1] Adams's transl. i. 195–6; Airs, Waters, and Places; Littré's trans. ii. 29.
[2] Praxeo's Med. ii. pt. ii. 315.

cases so noticed were, probably, in most instances, nothing more than severe catarrhal affections. In saying this, I trust I shall not be considered as undervaluing the merits of this truly extraordinary man; but, whatever may have been the remarkable powers of observation he possessed, it is not to be supposed that his diagnostical proficiency was such as to enable him invariably to discriminate with accuracy between catarrh and true parenchymatous inflammation of the lungs.

Dr. Forry, in his account of the climate of the United States, remarks of pneumonia, pleurisy, and catarrh, that they are "invariably less prevalent in the moist and changeable climate peculiar to the sea-coast and large lakes, than in the dry atmosphere of the opposite locality;"[1] and we all know that it is precisely in the former localities that fever abounds. In the northern division, the average number of cases of pneumonia in 1,000 was found to be 45; that of intermittent and remittent fevers, 231. In the middle division, the averages in 1,000 were 74.5 of pneumonia, and 739 of fevers; and, in the southern division, the average of pneumonia in 1,000 was 43, and of fever 598.[2]

In the East Indies, where jungle and remittent fevers are common, pneumonia is scarcely known. Desportes, while quoting the old aphorism, *callidum pectori amicum, frigidum inimicum*, informs us that the disease is less frequent in St. Domingo, one of the most malarial of the West India Islands, than in France.[3]

In the Windward and Leeward islands, as well as in Jamaica, where sources of malaria abound, and where fevers, in consequence, constitute near one-half of the prevailing diseases, pneumonia, so far from being a common complaint, is rarely encountered. In the former command, an average military strength of 4,333, gave, in twenty years, 1817–'37, 62,163. Of these, 24,607 were quotidians; 1,973 tertians; 133 quartans; 17,799 remittents, and 774 yellow fever. This makes a total of 45,286 malarial fevers, the balance being common continued (typhoid ?), typhus, and synochus. During the same period, inflammation of the lungs furnished a quota of only 1,941, and pleurisy of 34.[4] In the Jamaica command, with an average military strength of 2,578, the number of fever cases were no less than 46,922, or 910 per 1,000. Of these cases, not

[1] Climate of the United States, 359.
[2] *Ibid.* 233, 277, 284.
[3] Maladies de St. Domingue, i. 32.
[4] Tullock, Med. Statistics of Brit. Army, 7, 8.

less than 6,090 were intermittents, or 38,393 remittents, and 20 yellow fever; the total being 44,503 autumnal or periodic cases.[1] Dr. Rufz, in a communication made to Dr. Grisolle,[2] states, as the result of his personal observation, that pneumonia is very uncommon at Martinique.

"Although we encounter in Senegal," says Thevenot, "the greater number of the pathological changes incident to temperate climates, and although endemic diseases there differ in nothing but frequency and severity from our own, yet there are some which will command almost exclusively our attention, because they prevail much more frequently than all others. Remittent and intermittent fevers, dysentery, hepatitis and nervous colic constitute the most dangerous enemies of Europeans. At the same time inflammation of the thoracic organs, though not unknown, is rare, and in a list of 952 cases of disease, treated from July, 1837, to July, 1838, malarial fevers amounted to 509, while pleurisies and pleuro-pneumonias did not reach above 5."[3] At Bone, in Africa, as we learn from Dr. Maillot, pneumonia is an excessively rare disease; while malarial fevers, on the contrary, abound to an unprecedented degree. Of 3,765 cases of disease treated by him from 9th February, 1834, to 15th March, 1835, only 6 were affected with pleuro-pneumonia.[4] In Upper Canada, where malarial exhalations, and, as a consequence, autumnal and periodic fevers abound much more than in the lower section of that country, pneumonia prevails much less extensively. The proportion of the latter disease in Upper Canada, per 1,000, is noted by Major Tullock at 60; in Lower Canada at 30; while the proportion of intermittent fevers per 1,000 in the former was 178, and of remittent 12; and the proportion of intermittents in the latter was only 26 per 1,000, and of remittents 1.[5] In French Guiana, and especially at Cayenne, where malarial fevers are proverbially common, and the climate is justly considered in consequence as excessively insalubrious, thoracic inflammations are rarely encountered.[6] It is true that Campet tells us a very different story on the subject. But he admits that such

[1] Tullock, Med. Statistics of Brit. Army, 46, 47.

[2] Traité de la Pneumonie, 132.

[3] Tr. des Maladies des Européens, dans les pays Chauds, 232, 240.

[4] Traité des Fievres Intermittentes, 114.

[5] *Opera cit.* 27, B; 29, B.

[6] Ségond Aperçu sur le Climat et les Maladies de Cayenne, 1831, quoted by Grisolle, 133.

inflammations are principally rife among the slaves who do field work, are destitute of proper clothing, and sleep on planks in badly constructed huts, where, unless protected by large fires, they are apt to suffer during the low temperature of the night and break of day.[1]

Similar are the results obtained on the western coast of Africa, almost every point of which may be regarded as a hotbed of malarial fevers. From Major Tullock, we learn that the Sierra Leone command presented, in a total number of 5,489 cases of disease, 2,600 of fever. Of these, 948 were of intermittents, and 1,601 of remittents, which, with 51 of common continued, were in the proportion of 1,411 per 1,000 of mean strength. In the list, the affections of the lungs figured for 103, being in the proportion of 56 per 1,000; and pneumonia for 15, or 8 per 1,000.[2] In conclusion, I may here mention a fact, to which Grisolle has already called attention, that Monfalcon, in his treatise on marshes, though pointing out the frequency of pulmonary catarrh in paludal localities, nowhere enumerates pneumonia among the diseases peculiar to them;[3] and that Nepple, though asserting the fact in positive terms, disproves his own statement by showing, that in a total of 1,352 cases of disease, admitted into the wards of the Hospital of Montluel (Ain) during the course of four years, intermittent and remittent fevers furnished not less than three-sevenths of the whole, while pneumonia came in for only one-sixteenth.[4] In the Ionian Islands, with an aggregate military strength of 70,293, the cases of malarial fevers in twenty years, 1817–1836, amounted to 16,252, being in the proportion of 230.5 per 1,000, while the cases of pneumonia and pleurisy amounted to 2,272, or 31.2 per 1,000.[5]

Again, in some parts of the south of Spain, where acute inflammation of the lungs is rarely encountered, paludal fevers, on the contrary, are very prevalent, and constitute, with gastro-enteritic and hepatic inflammation, the great bulk of the diseases of the country.[6] Dr. Roux calls attention to the fact of the small proportion of thoracic affections, acute and chronic, in the Morea, where paludal localities extend far and wide, and where malarial fevers are necessarily abundant. "In France," he remarks, "at this season

[1] Maladies des Pays Chauds, 210, 211.
[2] Mortality of the British Army, 8–10.
[3] *Ibid.* 500.
[4] Nepple, Essai sur les F. Int. et Remit. 15, 297.
[5] Tullock, 32, 34, 35.
[6] Boudin, Geog. Med. 85.

of the year (January), if we were to collect an equal number of siek in a hospital ward, we should hear little else than the sound of cough, and many eonvaleseents would present, at the moment of their diseharge, the sequelæ of some thoraeie affeetion. Here, nothing of the kind oeeurs."[1] Few eountries suffer more from malarial fevers, intermittents and remittents, than the Island of Ceylon.[2] Statistieal returns of the diseases observed there among Europeans, show that pneumonia forms but a small item in the list, the number of eases being only $\frac{1}{613}$th of that of all diseases, aeute and ehronie.[3] Dr. Davy also states that diseases of the lungs are exeeedingly uneommon in that island, both among Europeans and natives. Asthma and pneumonia now and then oeeur (p. 491). Aeeording to Dr. Marshall, the proportion of deaths among the troops from all diseases and from pneumonia, from 1816 to 1820, inclusive, was as follows:—

	From all diseases.	From pneumonia.
1816	63	2
1817	57	7
1818	513	6
1819	310	6
1820	139	8

This gives us a total of 1,082 from all diseases, and of 29 from pneumonia, or 1 in 37.27.

During a period of three years, extending from 21st of Deeember to 20th of the same month, 1817–1818, 1818–1819, and 1819–1820, the number of eases among European troops treated in the hospitals of Badula and Kandy, were as follows:—

	Badula.	Kandy.
First Period.		
Fever, intermittent, ...	224	746
" remittent	163	337
" eommon eontinued .	000	104
Pneumonia	4	16
Second Period.		
Fever, intermittent	86	170
" remittent	94	83
" eommon eontinued . .	00	84
Pneumonia	2	00

[1] Hist. Méd. de l'Armée Francaise en Morée, 84.

[2] Davy: An Account of the Interior of Ceylon, 4to. 493. Marshall. Notes on the Med. Top. of Ceylon, 89.

[3] Grisolle, 130.

Third Period.	BADULA.	KANDY.
Fever, intermittent	20	11
" remittent	13	3
" common continued . .	00	108
Pneumonia	1	14

We have here, in three years, a total of 2,246 cases of endemic fevers of various types; while pneumonia figures in the list for 37, or 1 in about 61.

It is to be remarked that the few cases of pneumonia observed, occur principally, if not exclusively, among the Malay, Indian, and Caffre tribes; the Europeans, who are most liable to endemic fevers, being comparatively but little liable to the disease.[1]

But these facts must suffice. So far as they go, they seem conclusive; and with suitable deference to the authority of writers who entertain sentiments different from those which it is my object to uphold, I cannot think I hazard much when expressing the opinion that the impartial reader, after perusing what precedes, will acknowledge that it will puzzle them, talented and ingenious as some of them undoubtedly are, to reconcile those facts with the hypothesis set forth regarding the identity of pneumonia with malarial fevers. Let them, if possible, account for the aforesaid circumstances—the absence of malarial fever where pneumonia is rife; and the extensive prevalence of the former in localities where the latter is either not of remarkable frequency, or scarcely seen at all—the cessation of the one and continuance of the other—and say how all this could be brought about, if the inflammation of the lungs, in the disease in question, were due to the agency of the cause producing those fevers; in other words, if pneumonia were really and substantially nothing more than a peculiar form of remittent and intermittent fever. Were the etiological connection and pathological dependence such as maintained, we should expect to find that the cause, if diffused in such localities to an extent, and possessed of a degree of energy, sufficient to produce a large number of cases of pneumonia, or, as others would say, of the pneumonic form of autumnal fever, would also give rise to a greater or less number of cases of the other and more legitimate and characteristic forms of those fevers. This would be the more natural to anticipate, because the climatorial influences noted in those favored spots where fevers have never

[1] Notes on the Med. Topog. of the Interior of Ceylon, 92–129.

prevailed, or have long ceased to do so, are not different from those under the empire of which such fevers everywhere appear; or have remained the same amid all the changes that have occurred in regard to their prevalence;—the only difference consisting in the absence or removal of certain terrestrial or local conditions, which, whatever be the nature of their association with the ordinary forms of the supposed protean disease, have apparently nothing in the world to do with the existence of what is now maintained to be simply another form of the same. The result being different—pneumonia occurring where autumnal fever has never originated, or where, if it has done so, it now seldom if ever prevails—we are warranted in concluding that the causes of the two diseases are different; that the one may exist without the other; that when the two diseases show themselves at the same time, and in the same locality, two sets of causes necessarily exercise their baneful influences and produce, not one disease assuming different forms, and presenting different aspects, but two distinct complaints; and that, consequently, pneumonia cannot justly be held up as forming part and parcel of autumnal fevers, which, as regards etiology, are governed by very different laws, and influenced by very different agencies.

3. *The two diseases prevail in different seasons.*—The contrast between the two diseases is particularly striking as regards the seasons in which they mostly prevail. As the name usually affixed to malarial fevers indicates, the latter is, in our latitudes especially, a disease of autumn. It seldom appears before the middle or close of summer, and ceases on the accession of winter. Its existence in an epidemic form in temperate regions, is never known to occur in the spring of the year;—cases that occur in that season, or in winter, being accounted for without having recourse to the supposition of the development then of the efficient cause. It never shows itself in winter, and if it appear early in summer—which is seldom the case—the occurrence is only noticed under peculiar circumstances of atmosphere existing during the preceding months, and which invest these with the characteristic conditions appertaining to autumn. It is emphatically a disease of hot weather, requiring for its production a continuance for some time previous of high atmospheric heat. It appears, generally, some weeks after the hottest month; the period being retarded as we proceed

north. For the same reasons it may readily be understood to be a disease of hot latitudes, prevailing, as it does, violently and almost perpetually within the tropics, and ceasing long before we reach the polar circle. Dr. Drake, who has noted all these circumstances, has, from extensive observations, arrived at the conclusion that, with the decrease of yearly and summer heat, other conditions remaining unchanged, there is an abatement of fever. Taking the heat of summer alone into consideration, he thinks we may assume that a summer temperature of sixty degrees is necessary to the production of fever; that it will not prevail as an epidemic where the temperature of that season falls below sixty-five degrees; and that, if the other conditions favouring its production are deficient, it will cease before those reductions of temperature have been reached.[1]

In the Mediterranean stations, the admissions into the hospital, and the deaths, average nearly twice as high between July and October as during any similar number of months in the year.[2] In Spain and Portugal, the sickly season, in malarial districts, is from July to September.[3] The fever of Rome, in general, is held to begin with the great heats about the end of June. The Roman people have fixed on St. Peter's day as the exact period of its outbreak, and it reigns from then till it is put a stop to by the equinoctial or autumnal rains of September. The readers of Horace will recollect that, in his time, July was notorious for its insalubrity. "Adducit febres et testamenta resignat."

In this country and in Canada it commences in July or August, and ends, in the south in November or December, and in the north about October. In England, much the same results are obtained.[4]

In hot latitudes, where the heat is almost constant, the fever, if not constant also, commences earlier than in our latitudes, and lasts later when not put a stop to by unusual agencies. On the African coast it exists to some extent at all seasons; but is formidable from April to November, and especially rife from July to the last-mentioned month. In Senegal, thundershowers commence towards the close of April, or beginning of May, and continue to the middle of July, when the rains set in. This season (*hivernage*) lasts to the end

[1] A Systematic Treatise on the Principal Diseases of the Interior Valley of North America, 712.

[2] Williams, on Morbid Poisons, ii. 460. [3] Macgregor, Med.-Chir. Tr. vi. 387.

[4] Williams, *op. cit.* Watson, 445.

of October or beginning of November. It is at this period (the hottest of the year) that remittent fevers usually make their appearance.[1] In the West Indies, also, the period of the greatest liability is between July and December, when the hottest weather, combined with considerable moisture, prevails. In Bengal, fever commences in August, and continues till November.[2] In Ceylon, the months of June, July, and August are, in general, the period of the year when the greatest sickness prevails.[3] In a word, the epoch of appearance and disappearance may vary in different localities according to the situation of these, and their position relative to the equator, and the consequent modification in the period of the seasons; but everywhere endemic or autumnal fevers break out, or are most rife, during or shortly after the hottest weather.

So far as regards the yellow fever, the disease may still more appropriately be denominated one of hot weather; requiring, as it does, a higher average temperature during the summer months—not less than between 76 and 80 degrees (Fahr.). It manifests itself in no climate where the temperature is below that average, and ceases to appear long before we reach the limits assigned to ordinary periodic fevers. In our climate it has commenced as early as June. Such was the case in this city in 1799, and in Charleston in 1837. We even find that in the year 1732 it appeared in the latter city as early as May. In 1852, one death by yellow fever was reported in May, and another in June. In 1819, the disease broke out in this city about the end of June. Such was the case in New Orleans in 1848. But instances of this kind are rare, and when they occur, the progress is at first slow, and the disease presents more the character of a sporadic than of an epidemic visitation. More frequently, the fever breaks out about the middle or end of July, often in August, and sometimes as late as September. Thus it began its epidemic career in the course of July, in Philadelphia, in 1747, 1794, 1797, 1802, 1803, 1805, and 1820; in Norfolk, in 1800, and 1821; in Providence (R. I.), in 1805; at Brooklyn (N. Y.), in 1809; in New York, in 1799, and 1803; at Woodville (Miss.), in 1845; in New Orleans, in 1817, 1844, 1847, and 1853; Rodney, in 1847; Boston, 1798, and 1819. Our epidemics of 1699, 1762, 1793,

[1] Levacher, 48.
[2] Clark on Long Voyages, i. 116.
[3] Marshall, Topography and Diseases of Ceylon, 39.

1798, commenced in August, as did also those of Charleston, in 1732, 1739, 1745, 1748, 1849, 1852; of Baltimore, in 1798; of New York, in 1791, 1797; of Providence, in 1798, and 1800; of New London, in 1798; of Boston, in 1819; of Wilmington (Del.), in 1798, and 1800; of Natchez, in 1817, 1819, 1823, and 1825; of New Orleans, in 1819; of St. Augustine, in 1839; of Mobile, in 1819, 1843, and 1847; and of Vicksburg, in 1847. In Gibraltar, Leghorn, and the various cities of Spain where the fever has prevailed—as Medina, Sidonia, Malaga, Antiquar, Cadiz, Barcelona, Xeres, &c., the period of appearance has varied from July to September; never earlier nor later. But in Europe, as in this country, at whatever time the disease may break out, it ceases as an epidemic, or even altogether in October, November, or December.

In the West Indies, and on the African coast, it makes its appearance, as might easily have been foreseen from the greater precocity of the hot season, at an earlier period than it usually does in this country and in Europe; while in some years, owing to the greater encroachment of hot weather on the autumnal months, it breaks out later than it does in temperate latitudes—the period varying from April (or even earlier, as was the case at Granada, in 1793, when it showed itself in February) to the middle or close of September.

If we now turn to pneumonia, we shall find that the period of its prevalence is very different from that of the disease just referred to. Hippocrates, some three thousand years ago, and Areteus after him, pointed out the winter as the season in which the disease manifests itself most commonly, and subsequent writers have generally united in that sentiment. Sydenham, it is true, and after him the learned commentator of Boerhaave, affirmed, as the result of their observations, that the disease attains its maximum between spring and summer. But the statement has not always been confirmed by subsequent authorities in this country and Europe. According to Chomel, pneumonia is more particularly rife in winter, spring, and the commencement of summer, and is rarely seen at the close of the latter season and during the autumn. Of 97 cases observed by that writer and Louis, at the Charity Hospital of Paris, 81 occurred between February and August, and 16 only during the other months.[1] Of 357 cases recorded by Leroux, and cited by Grisolle,

[1] Dict. de Méd. xvii. 210, 1st ed.

116 occurred in winter, 137 in spring, 58 in summer, and 44 in autumn. Andral states that the disease prevails mainly in the spring months—March, April, and May.[1] The following are the results obtained by Grisolle in an examination of 296 cases:—

January	20	July	13
February	40	August	3
March	47	September	5
April	62	October	2
May	40	November	22
June	8	December	34

Admitting this to be a fair representation of the comparative monthly frequency of the disease among adults in Paris, it follows that pneumonia attains its maximum in April. Next come in order of frequency, March, February, and May; then December, November, and January; while August and October present the smallest number of cases.[2] Hourman and Dechambre, cited by Grisolle, who investigated the subject among the aged inmates of the Salpetriere, found the cases more numerous in March.[3] At Geneva, on the contrary, the disease often attains its maximum in May;[4] while among children, Rilliet and Barthez point out February as the month of greatest frequency—a result modified, doubtless, as Grisolle remarks, by the circumstance that the influenza was then existing in Paris. Facts very similar to those stated are recorded as being usually observed in England, in the more northern and southern districts of Europe, at Malta, in Bermuda, Nova Scotia, Canada, and in every part of this country and South America.[5] Everywhere, among adults, children, and old people, idiopathic or primitive pneumonia appears to attain the maximum of frequency at the close of winter and in the spring, especially during the months of March and April. Next in point of frequency is the winter; while the disease, though not unknown in summer, is comparatively

[1] Med. Clinique, i. 513.
[2] Grisolle, 137.
[3] *Op. cit.* and Archives, xii. 2d series. 30.
[4] Essai Statistique sur la Mortalité du Canton de Genève, Ann. d'Hyg. xxiii. 51.
[5] Williams, Cycl. of Pract. Med. iii. 407; J. Frank, 311; Andral, i. 513; Tullock, 44, B; Wilson Philip, Tr. on Symptom. Fever, 204; Briquet, Arch. Gen. vii. 3d s. 482; Ramsay, Charleston Journ. vi. 1, 2; Bell and Stokes, ii. 207; Smith on Epid. 80: Johnson and Harris, Trans. Am. Med. Assoc. v. 373; Gerhard, Amer. Journ. 1834; Ed. F. Williams, Stethoscope, ii. 544; Lafargne, Bulletin de l'Acad. xvii. 198

of rare occurrence during that season, and perhaps still more so during the autumnal months.

Grisolle mentions farther, that of thirteen epidemics of pneumonia, the authenticity of which cannot be contested, seven commenced in spring, four in winter, one in autumn, and one in summer.[1] The epidemics noticed in this country have usually commenced in winter, and prevailed in that season and spring, the latter often furnishing the greatest number of the cases.

4. *They appear under the influence of opposite winds.*—It is not to be forgotten that the two diseases do not usually manifest themselves under the influence of the same order of winds. So far as regards malarial fevers, not a little in this matter will depend on the nature of the surface over which the wind may happen to pass before reaching the locality where the disease prevails; for at the proper period of the year, other things being favourable, fever will appear, as we shall see, in connection with any currents which waft the air from neighbouring surfaces where the elaboration of the morbific cause is going on. Edinburgh is supposed to derive fever through the agency of the east wind, which blows it from Holland. The same wind wafts malaria from Essex to London. The north-east wind blows malarial fever into some portions of Rome. Generally speaking, the western coast of Italy contains a more extensive elaborating surface than the eastern, in consequence of which, other circumstances being equal, the western currents are more prolific of fever than their antagonists. The same thing, and for the same reason, occurs in Europe generally, from the frontiers of Asia to the other extremity of that continent. In Batavia, the north-east wind, which is very prevalent during July, August, and September, is highly unfavourable to health.[2] So are the north winds at New Orleans during summer,[3] and the east and west winds at the Havana.[4] In Normandy, at Berniere, an unusual course of south-west winds blowing across a pestiferous region, and conspiring with a hot summer, caused several violent epidemics.[5] In London, the south-east winds are apparently unfavourable for the same reason.[6] The same thing occurs in Dutch Guiana.

[1] *Op. cit.* 189.

[2] Horsefield, Med. Mus. i. 79.

[3] Report on Fever of 1819, p. 50; Chabert, viii. pp. 9, 26.

[4] Osgood, 29.

[5] Macculloch on Malaria, 351.

[6] Second Report of Lond. Commissioners, 40.

"Yellow fever," says Dr. Bone, "prevails on Brimstone Hill, St. Kitts, when the strong north winds that have swept foul ground on Mount Misere impinge upon the persons in the ill-constructed barracks and out-buildings on that hill. And in Tobago, Dominica, Grenada, St. Vincents, and in all the hilly uncleared islands of the West Indies, strong north and east winds and rain, impinging upon the troops and their families in ill-constructed barracks, are causes of the disease."[1] "In St. Domingo," says Desportes, "the acclimated inhabitants regard the north wind as unwholesome, while the south wind is very pernicious to new-comers. This is particularly noted in the plain of the Cape, because the north-east and north-north-east winds, before reaching it, pass over a large surface of marshy localities, from the effect of the exhalations of which the natives, who are proof against seasoning fevers, are not exempt."[2]

In this city and country, instances of the kind might be cited in abundance; but they, as well as many others, will find a more appropriate place in another part of the present volume.

But however true it may be, that particular currents of winds exercise a baneful influence in the way mentioned, it is not less a fact which experience will everywhere confirm, that the existence of malarial fevers has almost invariably been connected, in temperate regions particularly, with the prevalence, during a greater or shorter space of time, of southerly currents, and that the influence that these exercise is not necessarily, and in some forms of the disease is very seldom, dependent on the malarial nature of the localities over which they happen to pass. Nor could this well be otherwise; for these fevers require for their development the long continuance of a range of thermometrical heat, which could seldom be obtained during the prevalence of opposite currents. Under the influence of these winds, whether from due south, or from the neighboring points, S. E., E. S. E., or S. W., accompanied, as they are, with heat, and in many, if not most malarious regions, with humidity, fever originates and spreads to a greater or less extent; while from a change of these to other winds blowing from the opposite points of the compass, as N., N. W., or N. E., there results a beneficial change in the healthfulness of the infected locality;—a diminished prevalence, or even a cessation of the fever. In tropical climates, where the most frequent variations are from

[1] Essay, 15. [2] Maladies de St. Domingue, vol. i. 21.

E. S. E. to E. N. E., and where the wind never remains long at N. E. or S. E., and rarely blows from due north or due south, the prevailing current is always attended with considerable heat and moisture; and with few exceptions, when the wind blows from the west longer than usual, and when the heat is nevertheless very great, as at Martinique in 1839,[1] the prevalence of fever is very usually associated with south-east or easterly currents.[2] One of the writers to whom I have referred below, and whose ample experience and great accuracy of observation cannot be doubted, remarks on the subject: "The development of the yellow fever in the West Indies, in a great number of men at the same time, in different parts, at a distance from each other, on a level with the sea, or slightly above, on board vessels in port or at sea, coincides so exactly with the increase of heat and humidity, and with the prevalence of the south winds, that it is impossible not to recognize in these meteorological conditions the true cause of the epidemics of yellow fever. The effect here is intimately and necessarily connected with its cause. The production and extension of yellow fever under the prolonged influence of the south winds, is a fact observed by every one; so inevitable, indeed, that it can be predicted without fear of ever being mistaken. This action of the south winds is felt here by everything that breathes. They produce undefinable effects on our senses. We feel them in bed, or sitting at the desk; they enervate, cause oppression, and depress the spirits. To say in what these atmospherical alterations, the effects of which are so much to be dreaded, consist, and to seek to determine their specific nature, is doubtless a task beyond the faculty of man." "It is a fact, I cannot say it too often, that yellow fever is never developed in the same manner as other epidemics, but by and under the influence of the south winds. When this general cause fails, the yellow fever only exists in certain restricted places, or only a few sporadic cases appear here and there, as occurred in 1819 and 1820. There were only, in all, forty-two men attacked by the yellow fever in the Hospital of Fort Royal in 1820, and yet, notwithstanding, the garrison, including sol-

[1] Rufz, 29, 54.

[2] Desportes, 19; Lempriere, i. 17; Bally, 361; Gilbert, 12; Humboldt, 765; Rochoux, 113; Lefort, Du Quinquina et de la Saigneé, &c. p. 66; *Ib.* Mém. sur la non Cont. de la F. J. 9; Leblond, 81; Lind on Seamen, 67; Dariste, 33; Arnold, 26; Savaresi, 189, 242–3; Moreau de St. Mery, 716; Chisholm, 88; Chervion Rept. on Rufz, 45; Catel, 20; Olivet, 6; Vincent, 7.

diers, seamen, and military workmen, was very numerous; but in that year the wind did not blow from the south, or only occasionally, and never more than twenty-four hours at a time. It may, moreover, be remarked that this injurious influence of the south wind, or of winds from neighbouring points of the compass, is observed under the same parallels of latitude or in corresponding parts of our hemisphere."

In temperate regions, the concurrence of southerly winds would seem to be even more requisite than it is in the West Indies, because, in these, the long continuance of the degree of heat necessary for the elaboration of the malarial poison may and does exist, as is seen during some sickly season, without the aid of such currents; whereas, in the former region, the temperature seldom if ever attains the degree alluded to, or remains at it long, except under the influence of the south wind or its collaterals. Indeed, it is doubtful whether, in our latitudes, fevers, of a malignant character, particularly, have often been found to prevail under a very different condition of aerial currents. It was under the influence of the warm winds that the fever described by Hippocrates, and which is so closely allied to the worst forms of the malarial fevers of Algeria and our Southern States, appeared. The yellow fever of Andalusia, Cadiz, Carthagena, and other cities of Spain, has almost invariably shown itself during the prevalence, or after a prolonged continuance, of the Levanter, or east wind, which is both hot and damp. Such has also been generally the case at Gibraltar.[1] In Italy, the south and south-west winds are the usual attendants on fever periods.[2] In Sicily, matters are not very different.[3] In like manner, in this city, and other parts of the United States, the yellow and other forms or varieties of malarial fevers, usually, if not always, break out and prevail during the continuance of south or some other hot winds.[4] In a word, the production and continuance

[1] Tullock's Report, 4; London Quarterly Rev. lxvi. 135, 6; Vance's Rep. in Pym, 60; Fellows on Pestilential Fever, 13, 15, 469; Berthe Fievre J. 51; Pariset, F. J. de Cadiz, 96; Gilkrest, Cycl. of Practical Med. ii. 279; O'Halloran on Yel. Fev. of Spain, 14; Wilson, Fev. of Gibraltar, Chervin's Translation, 9; Rochoux, 113; Gilpin, Med.-Chir. Trans. v. 339; Bally, 363; Amiel, in Johnson on Trop. Cl. 260; Townsend's Spain, 340; Maclean on Epidemics, ii. 485.

[2] Carriere, Climat de l'Italie, 462; Valentin, Voy. Méd. 45; Baglivi, Opera Omnia, 157–8; Edinburgh Rev. xxxvi 542.

[3] Boyle, Edinb. J. viii. 178.

[4] Caldwell on Malaria, 135; *Ib.* Med. Repos. vii. 144–5; Barnwell, 366: S. Jackson, 40; Waring, 23; N. O. Rept. for 1839, 156; La Révue Méd. for 1840, 322: Rand.

of autumnal fevers—periodic and yellow, is usually connected with the prevalence of southerly winds; and on inquiry it will be found that in instances when a different result was obtained, the thermometer had nevertheless ranged high.

Now, how do matters stand in that respect as regards pneumonia? If, in some localities, as at Gibraltar, for example, the disease prevails most usually during those periods of the year when westerly winds are predominant; if the disease also is found to occur more frequently in other places under the influence of different currents, it may be laid down as a general rule, that north, north-east, and north-west winds are those during the prevalence of which thoracic inflammations are more frequently developed. Such was found to be the case in the north of Italy and Germany by J. Frank.[1] Le Pecque de la Cloture[2] made similar observations in Normandy, as did also Hourmann and Dechambre,[3] and Grisolle,[4] at Paris. Of fifty-four cases of the disease observed by the latter author, fifteen occurred with a north-east wind; eleven with north-west; three with north wind; five with south-east; eight with south-west, and eight during a perfect calm. Hippocrates, long ago, noted the prevalence of pneumonia under the influence of north and north-east currents in Greece. "But the following is the condition of cities which have the opposite exposure, namely, to cold winds, between the summer settings and the summer risings of the sun, and to which those winds are peculiar, and which are sheltered from the south and the hot breezes." "The diseases which prevail epidemically with them are pleurisies, and those which are called acute diseases."[5] Similar results have been noted in England,[6] in Nova Scotia, and Bermuda; and it can scarcely be necessary to add that in this country pneumonia is almost invariably associated with the prevalence of northerly currents—N., N. E., N. W., or west.

5. *Pneumonia is of yearly occurrence—not always so fevers.*—In connection with this subject, it may be mentioned that in places lia-

Med. Repos. ii. 466; Valentin, 85–6, 89; Fev. of Baltimore in 1819, 95; Archer, Recorder, v. 61; Bailey, 55; Townsend, 55; Merrill, Phil. J. ix. 233; Barton, Fev. of N. O. in 1833, p. 9.

[1] *Op. cit.* 312.

[2] Malad. Epid. 1st pt. 15.

[3] Arch. Gén. 2d s. xii. 30.

[4] *Op. cit.* 141.

[5] Airs, Waters, and Places; Syd. Soc. Ed. i. 193; Littré's Tr. ii. 19–21.

[6] Cyclop. of Pract. Med. iii. 407.

ble to pneumonia, the appearance of the disease is of yearly occurrence. The cases may fluctuate in respect to their number. There may be comparatively few one year and many on another occasion. At other times, the disease may assume the character of an epidemic. But nowhere and on no occasion has it been known to disappear during the whole of the season at which it usually prevails, or during an entire year; still less has it been known to fail entirely during a series of years, and to recommence its course as heretofore after a long period of repose. Nor does it suddenly visit localities to which it had before been a stranger; and, after scourging them for one or more seasons, disappear and be seen no more. The disease, being the offspring of causes which, as we shall see by and by, are strictly local in their character and dependent on particular states of the sensible qualities of the atmosphere, which do not differ very materially in the same localities in successive years, must, and does consequently, show itself more or less extensively at each return of its appropriate season without being much, if at all, influenced in the frequency of its manifestation, far less prevented entirely, by modification in the condition of the soil or of surrounding objects.

But while such is the case in relation to pneumonia, a different result obtains in regard to autumnal or periodic fevers of various grades and types. Every one knows in this country that although the bilious remittent fever, the endemic of many localities from one extremity to the other of our vast republic, appears in such places to a greater or less extent, every year at stated periods, seasons occur at which it does so in so trifling a degree as to attract little notice; and others again when it does not show itself at all. On the other hand, it is a fact, placed beyond the reach of doubt, that the disease is sometimes observed to spread extensively over a large expanse of country, attacking places that had been for several or many years almost or completely exempt from its ravages. Such was the case in 1822, and a few succeeding years, when the disease swept over immense tracts of country, in many portions of which it had not been heard of for many years before. Similar observations have been made in different parts of Europe, England, France, Spain, Italy. In the West Indies, many facts might be adduced in proof. Chisholm, in the 18th volume of *Duncan's Medical Commentaries*, describes an epidemic of remittent fever which occurred in the vicinity of St. George, Grenada, in 1792; on which occasion the

disease spread in an unprecedented manner, and attacked spots heretofore viewed as salubrious. The following remarks by Dr. Imray, in relation to the endemic of Dominica, will apply equally to the fevers of other islands: "The causes of our endemic fevers reside permanently in some localities; but in certain seasons, remittent and intermittent fevers become prevalent throughout the island." "Though the causes from which periodic fevers originate are permanent in the country, yet do they not always operate with the same energy. Sometimes these morbific agencies remain in almost a quiescent state, and fevers occur comparatively rarely. At other times, they manifest unusual activity, and then we have remittent and intermittent fever in abundance."[1]

Still more striking are the intervals observed between the epidemic manifestations of yellow fever, the elaboration of the cause of which is of much less frequent occurrence than that of ordinary autumnal pyrexia. When the disease broke out at Charleston in 1792, it had not prevailed there during the long period of 37 years. It did not reign in that city from 1807 to 1817, from 1830 to 1838, or from 1839 to 1849, and has seldom at any epoch, from 1690 to this day, prevailed in two successive years.[2] In New York, it has shown itself often at long intervals; the years of epidemic there having been 1741, 1743, 1751, 1762, 1791, 1795, 1796, 1798, 1804, 1805, 1819, 1822. In this city, it did not prevail from 1699 to 1742. It next showed itself in 1747, 1749, 1762. From the latter year to 1793, a period of 31 years, there was a complete repose. The disease again showed itself in 1794, 1797, 1798, 1799, 1802, 1803, and 1805, and lastly in 1820. From the last-mentioned year to the present, 1853—when the fever again made its appearance, in a mild epidemic form, the number of cases not having reached above 170—the city remained free from it. During some of these intervals, sporadic cases occasionally, but very rarely, presented themselves. Kindred occurrences have been observed in Boston, New Orleans, Mobile, Baltimore, and others of our cities; in some of which, indeed, the disease has occurred on a few occasions only. When it broke out at Cadiz, in 1800, it had not been seen there for 36 years; the last visitation being that mentioned by Lind as having occurred in 1764. Barcelona, which suffered so

[1] Observations on the Character of Endemic Fever in the Island of Dominica, Edin. Journ. lxx. 284.

[2] Simons, Charleston Journ. vi. 779.

severely in 1821, had not been visited by it since 1803. It had not been seen at Gibraltar for a century, at the time of its outbreak in 1804. At Vera Cruz, the city was free from 1776 to 1794. If we except Sir W. Pym, and a few others of the same school, physicians who are conversant with the yellow fever of the West Indies, whether they be contingent or absolute contagionists or anti-contagionists, have, from the days of Towne and Desportes to our own, admitted and continue to admit that the disease, such as it exists there, is a native of the soil, and prevails more frequently than anywhere else. And yet there is not one among them who does not know that the fever is not of annual occurrence in those islands. Sporadic cases doubtless occur frequently during some seasons. At others, however, they are seldom seen; and at some again, they are almost, if not altogether, unknown. If we open Desportes's excellent work on the *Diseases of St. Domingo*, we shall find that in the course of the fourteen years he resided at the Cape, the place was free from the fever during five.[1] At Martinique, yellow fever did not appear from 1827 to 1838. Grenada, when visited in 1793 by the epidemic described by Chisholm, had not suffered for 31 years. Dominica was also exempt from 1817–1821 to 1838. Georgetown, Demerara, suffered severely in 1793; next, in 1803; then in 1819. From that year to 1837 the disease did not show itself in the epidemic, if indeed in any form.[2]

We find malarial fevers starting up even in times not characterized by the existence of an epidemic constitution; sometimes from a slight change in the local condition of the soil or of surrounding and sheltering objects, in spots where it had not been seen before. Fever has for years gradually impinged on the city of Rome. It is to this day spreading, and every year reaches some parts of the city where it was before unknown.

On the other hand, from the operation of causes which it is not necessary to enumerate, fever, as we have already seen, diminishes or even disappears from localities where it had before prevailed more or less extensively.

6. *Their altitudinal range is not the same.*—Every one acquainted with the habitat of autumnal or periodic fever must be aware that

[1] Chervin, De l'identité des Fièvres d'origine Paludéenne de différens type, 5–33.

[2] Frost, Med. Repos. 12 and 13; Blair, Some Account of the Last Yellow Fever Epidemic of British Guiana, 28.

its cause is not exhaled, or, if so, is inoperative beyond a certain degree of elevation above the level of the sea; an effect due, like that resulting from Northern latitudes, to the absence of the average of temperature required for the elaboration of the cause in question, or for rendering the system predisposed to its impress. But whatever be the elevation at which it shows itself, its usual places of abode are valleys, plain surfaces, the banks of streams, lakes, and the like; while it very commonly spares high and well-aired situations. If it attack these—which is occasionally the case—the effect is often due to the cause being wafted thither by the wind, and sometimes, though more rarely, to the existence in the vicinity of sources of morbific exhalations, or of a soil which from its natural constitution is calculated, independently of true marshy conditions, to give rise to such exhalations. As regards the yellow fever, the altitudinal range of which is below that of ordinary autumnal or periodic fevers, it has never been known to appear on high, and rarely on hilly situations. Indeed, it has seldom, if ever, shown itself in any other than localities of a very opposite character. On this subject, however, I shall have occasion to dwell in detail in the next chapter, and need not say more about it at present.

If, with these facts before us, we inquire how matters stand in relation to pneumonia, we shall find that, on this point, as on the several others already examined, the disease differs widely from autumnal and periodic fevers. For, while the latter cease to appear in localities situated at a certain degree of elevation above the level of the sea, in the same way as they do at certain degrees of latitude, no elevation of the kind has as yet been pointed out as forming a barrier to the production of pneumonic inflammation. Let fever stop as an epidemic, or endemic, or as a sporadic disease, where it may, the inhabitants of places where the cessation occurs, or of those situated beyond that limit, continue to suffer from thoracic inflammation, just as extensively and severely as those below, if, indeed, they do not to a greater extent. While fevers, whatever be the elevation above the level of the sea at which they prevail, make their appearance principally in the localities specified, and seldom, except under special circumstances, break out in high and well-aired situations, pneumonia, though not a stranger to low grounds, valleys, and the like, prevails as widely, if not more so, on mountains, hills, and other similar spots. To prove this by a reference to a

large number of examples, would be a work of supererogation; for there can scarcely be found a physician who is not perfectly conversant with the fact. We know how subject the low grounds and miry river banks of Africa are to fever. On elevated localities, which in some places are comparatively free from that disease, the inhabitants are sufferers from pneumonia. This is the case in the island of Fernando Po, and other places.[1] Grisolle, though not prepared to admit the injurious effects of great terrestrial altitude in the production of pneumonia, cites the instance of a body of French troops encamped from the 15th of December to the 15th of May, on Mount Cenis, where malarial fevers are not common, if at all known. Among these troops, pneumonia constituted one-fourth part of the diseases from which they suffered.[2] During the year 1850, pneumonia in children prevailed to a considerable extent in all its various forms in West Philadelphia. Dr. Pugh, who reports the occurrence, adds: "And I think I may say, at least as far as my practice or knowledge extended, that the disease was not influenced by locality, but was quite as common on high as *low* grounds." The same physician remarks, that fever in the same season extended from the locks of the Schuylkill Navigation Company to Chestnut Street, and about five squares *West* of Market Street Bridge. "I think it may be asserted with safety that the farther we go West, or the more elevated the ground becomes, the less of that malady will be found."[3] Here then we see, in the one case, pneumonia prevailing most extensively on one of the highest ridges of the Alps, where fever does not appear, and in the other, while fever ceases in proportion as we ascend from the Schuylkill, pneumonia is not influenced in its frequency by the change of locality. Not different are the results obtained in other elevated and mountainous regions of this country. "The elevation and coolness of the Alleghany," says another reporter, "secures the inhabitants entirely from the fevers peculiar to those of the first-mentioned division (that comprising low alluvial land, bordering the Juniata River), while they suffer most from those of a pleuritic, pneumonic, and rheumatic character."[4]

Dr. Williams[5] remarks: "Elevated districts are perhaps the more

[1] Daniel, Topogr. and Dis. of Guinea, 137–453.
[2] P. 135.
[3] Transact. of Pa. State Med. Soc. i. 47.
[4] Rep. of the Blair Cy. Med. Soc. Tr. St. Med. Soc. i. 105.
[5] Cyclop. of Pract. Med. 3, 407.

liable to pneumonia because they are more exposed and colder, whereas the humid air of low valleys, while it diminishes the intensity of the cold, relaxes more the mucous surfaces, and renders them the weaker points of the circulation."

7. *Fevers influenced by nature of soil—not so pneumonia.*—Autumnal or periodic fevers, as every medical inquirer must know, are found to originate mostly, if not exclusively, in localities the soil of which presents peculiar geological characters, and is composed, in great part at least, of organic elements of a particular kind, or contains on its surface, or at a short distance beneath, substances more or less foreign to its composition, but giving rise, when acted upon by certain thermometrical, hygrometrical, and other influences, to the evolvement of poisonous exhalations. Experience has shown, also, that the cause of such fevers, whatever be its nature, possesses different degrees of affinity for the many substances over which it passes when wafted by aerial currents; that these substances act, as it were, as so many attracting or repelling causes, tending to limit or extend the sphere of its morbid agency; and that hence while fever, the efficient cause of which is evolved at some distance, abounds on a given stratum of soil, it spares individuals residing on strata of a different character. What the peculiarities of localities thus afflicted are, will be stated in a subsequent part of these inquiries. Suffice it here to say, that, wherever they are found to exist, hygrometrical, thermometrical, and other influences aiding, fevers will also be found to originate, and often to abound. As regards pneumonia, I am not aware, and greatly doubt, that any facts entitled to our respect have as yet been pointed out calculated to connect the origin or prevalence of the disease with any geological formation, the peculiar composition of the soil, or the particular substances which may be spread on or placed beneath the surface of the latter.

8. *Fevers arrested by frost—not so pneumonia.*—Nor is it less essential to remark, in opposition to the views under examination, and in support of those here maintained, that in places where malarial fevers prevail during a certain period of the year, they are almost suddenly put a stop to, whatever be their forms or types—intermittents, remittents, and yellow—by the advent of frost. After the

occurrence of the latter, localities which before were unhealthy—to such an extent as not to be visited with impunity, especially at night—become free from such fevers, and from all diseases of a malarial origin.

Now, as by the thermometric change in question, the morbid agent that had given rise to autumnal fevers has been destroyed, as proved by the cessation of its legitimate effects, and as, while this takes place in regard to fever, pneumonia is undisturbed as to its prevalence, or, indeed, is even increased in point of frequency, it is natural to conclude that the cases of pneumonia which continue to show themselves as heretofore, after the occurrence of that change, cannot be referred to the baneful agency of the cause in question, but are due to the operation of some other morbific influence, over which frost exercises no control, and differing, consequently, in its nature, from the former. If we admit this, we cannot err in admitting, also, that the same causes which gave rise to cases of pneumonia, occurring *after* a stop has been put to autumnal fevers by frost, must have occasioned the cases of that disease observed *during* the prevalence of such fevers and anterior to the advent of frost. If the cause which produced pneumonia *after* that event must, for reasons stated, differ essentially from that occasioning the fevers whose career has thus been arrested, the cause giving rise to the cases that appeared *during* the fever season must also be different from the febrile poison; and we hence arrive at the conclusion, already adverted to, that, when the two diseases show themselves together in the same place, two different sets of causes are at work, and from this difference of cause we have reason to infer the existence of a difference in the nature of the diseases produced; and that, when pneumonia is followed by, or succeeds to, fevers, it is not influenced in its production by the causes to which these are due.

I am not ignorant of the fact that the salutary effects here ascribed to frost, of putting a stop to autumnal fevers, has been called in question. I am aware that, in proof of the fallacy, it is remarked that such fevers are encountered every month of the year; that they prevail, more or less, all the year round; and, consequently, I am prepared to be met with the objection that, such being the continued prevalence of the disease, in cold as well as in warm weather, any argument against the etiological and pathological connection of pneumonia with autumnal fevers, founded on the supposed cessation

of the latter through the effect of a reduction of atmospheric temperature to the freezing point, must be set aside.

How far such an objection will influence those who have paid due attention to the subject, I need not inquire. Having, however, adduced the fact of that cessation, as one well established and militating strongly against the views under examination, I must be allowed to show, were it only for the benefit of those who venture on the denial, that what I have said in the matter is not a dream of the imagination—"a sheer assumption, worthy of one who is behind the times"—but has received the sanction of the highest medical authorities, in all parts of the world. Now, that in hot or very mild climates, malarial fevers may, and often do, continue to prevail, more or less, all the year round, is a position which, as every one conversant with the subject knows, will admit of no doubt. But the fact of such a continuance of the disease in those climates does not in any way impair the correctness of the statement made respecting the aforesaid power of frost; the explanation of the difference being easily found in the circumstance, that in those climates the cold of winter does not prevail in sufficient force, or continue long enough, to produce the effect contended for, and decided frost never or seldom occurs at all. But, be this as it may in regard to the climates mentioned, it is indubitably true, that whenever, in a miasmatic district, the thermometer descends to the freezing point, and there remains awhile, the progress of malarial fever is arrested. In this vicinity, among others, every one, from the learned physician to the gossiping granny, knows full well that the accession of severe cold weather, and particularly of black frost, is sure to be marked by a cessation of the fevers in question, and that, in the few cases that present themselves after that event, the attack is ascribable to that power of dormancy, by virtue of which the cause of some diseases remains, for a greater or less length of time, latent in the system after exposure, and which, in some forms of fever, may be prolonged for weeks and even months. Had I space and leisure to enlarge much on this subject, and, indeed, were it necessary, I could demonstrate, by means of numerous quotations, that such is the case also in other parts of this country, and in foreign lands—in every place, indeed, where the cold of winter is well marked or the frost severe. A few references will be sufficient. Thus, in regard to periodical fevers, the fact of their cessation, through the effect of cold or frost, is recorded in the writings of the physicians

of our northern and middle States, and of Canada: Rush,[1] Currie,[2] Potter,[3] Caldwell,[4] E. H. Smith,[5] J. M. Smith,[6] Wood,[7] Coventry,[8] Vaughan,[9] Lee,[10] Usher Parsons,[11] Agnew,[12] Lucas,[13] Stratton.[14] In the South, South-west, and West, we find it mentioned and insisted upon by Taylor,[15] Ramsay,[16] Hansford,[17] Thompson,[18] Waring,[19] Arnel,[20] Prior,[21] Smelt,[22] Dunbar,[23] Staley,[24] Dickson,[25] Hildreth,[26] Drake,[27] Simons,[28] Grant,[29] Cooke.[30] Cross we the Atlantic, we find the same thing taught by Sydenham,[31] Lancisi,[32] Cleghorn,[33] Bartholin,[34] Bancroft,[35] Williams,[36] Copland,[37] Macculloch,[38] Monfalcon,[39] Rigaud de Lisle,[40] R. Hamilton,[41] Sir J. Pringle,[42] F. Home,[43] G. Brown,[44] Tournon,[45] Bailly,[46] Sir Ch. Morgan,[47] Sir James Clark,[48] Booth,[49] Carriere,[50] De Renzi,[51] Jacquot.[52] In fact, it is mentioned

[1] Non. Cont. of Yellow Fever, Med. Repos. vi. 162.
[2] On Bilious Fever, 13, 21; Med. and Philos. Register, i. 181, 195.
[3] On Contagion, 16.
[4] On Miasma, 16; Med. and Phys. Mem. (1800), 202.
[5] Webster's Collection, 107.
[6] On Epidemics, 71.
[7] Pract. of Med. i. 142, 267.
[8] On Lake Fever, N. Y. Med. and Phys. J. iii. 15; Tr. of Med. Soc. of the State of N. Y. for 1825, 42.
[9] Med. Repos. iv. 130.
[10] Med. Repos. iii. 252.
[11] On Malaria (Essays), 200.
[12] Med. Recorder, vi. 138.
[13] Med. Recorder, v. 420.
[14] Edinb. Med. and Surg. J. lxiv. 105.
[15] Webster's Collection, 148.
[16] History of South Carolina, ii. 55–6.
[17] Webster's Collection, 148.
[18] Chapman's Journal, x. 106.
[19] N. A. Med. and Surg. J. i. 5; ix. 375.
[20] Med. and Philos. Register, ii. 10.
[21] In Potter, *op. cit.* 16.
[22] Med. Repos. ix. 127.
[23] Med. Repos. viii. 258.
[24] Med. Recorder, v. 460.
[25] Am. J. ii. 64.
[26] Med. Repos. xi. 345; Chapman's J. ix. 109.
[27] Topog. and Dis. of the Valley of the Miss. 608, 713.
[28] Charleston J. iv. 543.
[29] Am. J. July, 1853, 112.
[30] Med. Record, vii. 453.
[31] Works, ii. 191.
[32] De Noxiis Paludum Effluviis, 46.
[33] Dis. of Minorca, 133.
[34] Hist. Anatomicarum, 72.
[35] Tr. on Yellow Fever, 292, 407; Sequel to do. 91.
[36] On Morbid Poisons, ii. 460.
[37] Vol. i. 759; ii. 1100.
[38] On Malaria, 155.
[39] Traité des Marais, 345.
[40] In Johnson on Trop. Cl. 313.
[41] On Marsh Remit. Fever, 28, 32.
[42] Dis. of the Army, 14, 116, 172.
[43] Med. Facts and Observations, 46, 76.
[44] Cyclop. of Pract. Med. ii. 235.
[45] Etudes sur Rome, i. 203.
[46] Traité des F. Int. Pernicieuses, 134.
[47] Lady Morgan's Italy, i. 439, ii. 106.
[48] Med. Notes on Italy, 80–3.
[49] Life of Dr. Armstrong, i. 258, ii. 295, 597.
[50] Du Climat. de l'Italie, 371.
[51] Oss. Sulla Topog. Med. del Regno di Napoli, 68; Miasmi Paludosi, 21.
[52] Des F. a Quinquina, 38.

as a well-known and indisputable phenomenon, by all who have written on the periodic fevers of England, Holland, Flanders, France, Italy, Spain, etc. Nay, in the West Indies themselves, the healthy season is the cool season, and the continuance of periodic fevers during the winter months is in direct proportion to the continuance of heat. In warm winters fever continues; in cool ones it disappears in great measure, or completely. The researches of Dr. Drake, in reference to the Valley of the Mississippi, show that, with the decrease of yearly and summer heat, other conditions continuing unchanged, there is an abatement of the fever; that a summer temperature of 60° is necessary to the production of fever, and the latter will not prevail as an epidemic when the temperature of that season falls below 65°; finally, that if the other conditions favouring its production are deficient, it will cease before those reductions of temperature have been reached. "According to these conclusions, the fever will occur in winter, at all places where that season has a mean temperature of 60° or upwards, as at Vera Cruz, Tampico, Havana, Key West, Tampa Bay and Fort King, and it is well known that cases do occur at those places, in that season; but in other places, where the winter heat barely rises over 60°, they are few in number. At New Orleans, and generally under the 30th parallel, where the mean winter heat is as low as 50°, the fever is suspended."

Dr. Drake farther says: "But the seasons are made up of months, and we are here brought to consider its connection with their respective temperatures. Up to Tampa Bay, every winter month rises above 60 degrees; but at New Orleans, or the 30th parallel, only the nine months from March to November have that temperature; and as we advance to the north, the number of months having it constantly decreases. Thus, at St. Louis, it is attained by five months only, from May to September inclusive; at Fort Snelling, by four; at Fort Brady, by three; at Montreal, by four; at Quebec, by three. In advancing farther north, June and September fall below it; and finally, in the distant north, July and August, or the entire year. Long before this reduction is reached by those two months, however, the fever ceases; and, therefore, it results that a continuance for more than two months of a heat equal to 60 degrees is necessary to the development of the fever." "It appears, from all that has been said, that within the tropics autumnal fevers occur throughout the year, and that, as we move northerly, the

duration of its prevalence shortens, by its beginning later in spring, and terminating earlier in autumn. March and November first escape, then April and May on the one hand, and October on the other—lastly, June and September."[1]

So much for endemic periodic fever. If we turn to the yellow fever, we shall find that kindred results are obtained. That in some sickly seasons, in this part of the country as well as in Europe, cases are sometimes met with several days, or (as in the South) some weeks after cold weather has set in, is true. But such results are rare, in the North particularly; and they are most generally the effect of the breaking out of the disease in persons who had imbibed the seeds of it prior to the fall of the thermometer. But here as elsewhere—at the North and the South—the disease invariably ceases as an epidemic, after a frost sufficiently severe to kill the leaves of trees and annual plants.

Dr. Rush,[2] speaking of our yellow fever, says: "It is completely destroyed by frost." Dr. Currie[3] remarks, in reference to the epidemic of 1793: "As the weather became cold and frosty, its declension was so rapid that it appeared as if extinguished by a miracle." In 1797, the Academy of Medicine[4] derived an argument in favor of the identity of yellow and bilious fevers, from the fact that they were both uniformly checked and destroyed by the same causes—heavy rains and frost. And if the reader will examine the records of all the other epidemics by which this city has been visited—those of 1699, 1741, 1747, 1794, 1797, 1798, 1799, 1805, and 1820—he will find that they were all put a stop to, more or less suddenly, by the occurrence of similar changes of temperature.

Drs. Harrison,[5] Thomas,[6] Gros,[7] etc., of New Orleans, inform us that the fever of that city ceases as an epidemic after the occurrence of frost. Drs. Moultrie,[8] Lining,[9] Campbell,[10] Chalmers,[11] Ramsay,[12] Harris,[13] of Charleston, tell us much the same thing, relative to

[1] Drake, 714.

[2] Facts intended to prove the Yellow Fever not to be contagious, Works, iv. 155. See also vol. iii. 98–9, 100, 201; vol. iv. 8, 45, 95.

[3] P. 2.

[4] Letters to the Governor of Pensylvania, etc. 3.

[5] Remarks on the Yellow Fever, New Orleans Journal, Sept. 1845, 130.

[6] Essai sur la F. J. d'Amérique, 110.

[7] Rapport, etc. 6, 61.

[8] French Translation, 5.

[9] Essays and Observations of Edinb. ii. 409.

[10] See Watts, 249.

[11] Climate of S. C. ii. 60.

[12] Med. Repos. iv. 219.

[13] Barton's Journal, ii. 29.

the fever of that city, which invariably ceases on the accession of frost or severe cold. Nor do we find matters take a different turn in that respect at Natchez. Of the epidemic of 1817, we are told by Dr. Perlee,[1] that "on the 9th of November there occurred a severe frost, which at once arrested its progress, and permitted the inhabitants to return in safety to their homes."

The same writer, in his account of the epidemic of 1819, informs us that the weather became cool in the middle of November, and the disease began to subside. About the first of December, there having been a moderate frost, the Board of Health quickly informed the inhabitants that they could return to their homes with a reasonable prospect of safety.[2] In reference to the epidemic of 1823, the most disastrous by which that city was visited, Dr. Merrill remarks: "During the night of the 31st of October, a very great change took place in the weather. The wind changed suddenly from the south to the north-west, and the thermometer fell from 78 to 28 degrees in about sixteen hours. On the 1st of November it was considered safe to return to the city."[3] In testimony of the salutary effect of frost on that occasion, we have the farther authority of Dr. Monette.[4] In 1825, the disease continued, with little abatement, until the 26th of October, when, as Dr. Merrill[5] states, "we had a heavy shower of rain. The next day the wind changed to north-west, and blew strong and cold, and during the succeeding night the thermometer fell to 34 degrees, which induced many families to return to the city."[6] Dr. Monette also remarks that the disease continued its ravages until checked by frost and cold weather, about the 28th of October.[7] From the same writer we learn that, in 1837, "the disease continued to spread gradually, and with occasional abatements, until checked by frost, about the 25th of November."[8] It may not be improper to remark that Dr. Merrill says, in relation to Memphis, that if the grading there "has fallen short of creating an epidemic of quite as grave a character as similar causes have elsewhere, it may be owing to the modifying influences of a few timely showers of rain or an early frost."[9] I have now before

[1] Philad. Med. and Phys. Journ. iii. 6. [2] *Ibid.* 10.
[3] *Ibid.* ix. 255. [4] Observations, 65.
[5] *Ibid.* Essay, 59. [6] N. A. Med. and Surg. Journ. ii. 220.
[7] Essay, 62; Observ. 67. [8] Observations, 70; Essay, 75.
[9] Public Address on the Health and Mortality of Memphis, Memphis Medical Recorder, i. 90.

me a list of about ninety good and substantial authorities, who describe the yellow fever as it has appeared epidemically in Boston, New York, Providence, Wilmington, Baltimore, Norfolk, Franklin, Vicksburg, Alexandria, Galliopolis, Gibraltar, Barcelona, Cadiz; who all, like the authorities already mentioned, in connection with the disease as it has shown itself in this city, New Orleans, Charleston, and Natchez, impart to us information which I recommend to the particular notice of those who deny the destroying and purifying agency of frost, of severe cold, or blasts of northerly winds.[1] I must also recommend to their attention those instances,

[1] Dalmas, 39; Caldwell (Fever of 1805), 51; *Ibid.* Memoirs (1800), 209; ditto of 1826, 124; Bally, 313, 314; Campbell, in Watts's Med. and Surg. Reg. 249; Moultrie, French Trans. 5; Monette, 1st edit. 50, 60, 63; *Ibid.* 2d edit. 7, 63, 65, 67, 70; Thomas, 110, 2d edit. 14; Potter on Contagion, 25; Osgood, 17; Hosack, Febrile Contagion, 10; Facts, etc., by the College of Phys. 4; Dict. des Sc. Méd. xv. 357; Davidge, 69; Caisergue, 194; Valentin, 88; Harrison, New Orleans Journ. ii. 130; Chalmers, Climate of South Carolina, ii. 60; Bancroft, 292, 407, 425; *Ibid.* Sequel, 89, 91; Selden and Whitehead, Med. Repos. iv. 129, 336; Chisholm, Letter to Dr. Haygarth, 177; Currie, Med. Register, i. 181; Report of Philad. Acad. of Med, 7; Med. Rep. i. 406; Hardie, Fever of New York in 1798, 13; *Ibid.* 1822, 67, 68; Condie and Folwell, Fever of Philadelphia in 1798, 81; Currie, Fever of 1799, 25; *Ibid.* on Bilious Fever, 15; Halphen, 62; Brown (S.), 26, 108; Pierquin, 40, 60; Townsend On Black Vomit, 30; *Ibid.* Fever of New York in 1822, 257–8; Palloni, Sulla Febbre Gialla, etc. 20; Dariste, 31; Jackson (Sam.), Fever of Philadelphia in 1820, 24; Letters on the Fever of Baltimore in 1819, 80, 113; Addom's Dissertation, 7; Hosack's Essays, i. 292; Shecut, 94, 100; Copland, iii. 169; Report on Fever of N. O. in 1820, 6; New York Report of Quarantine, 44; Stone, New Orleans Journ. ii. 551; Pym, 2, 8, 216; *Ibid.* 2d edit. 2, 65; Smith (J. M.), 81, 90, 91; Burnett (Sir W.), 342; Blane, (Sir G.), Dissertations, ii. 152, 155; Caillot, 107; Barton (Ed.), Fever of New Orleans in 1833, 6, 9; Chervin, Report on Mem. by Rufz, 77; *Ibid.* Letter to Monfalcon, 17, 18; Forry, Climate of U. S. 290; Bayley, Letters from Health Office, 9; Amiel in Johnson on Trop. Cl. 270; *Ibid.* Edinb. Med. and Surg. Journ. xxxv. 276; Robert, Guide Sanitaire, i. 43; Physical Inquiry, etc. (N. Y.), 25; Gillkrest, Cyclop. of Pract. Med. ii. 279; Lining, Edinb. Essays, ii. 409; Smith, Edinb. Med. and Surg. Journ. xxxv. 40; Wood, i. 296; Chapman, Med. and Phys. Journ. ix. 135; Merrill, *ibid.* ix. 255; *Ibid.* North Am. Med. and Surg. Journ. ii. 220; Townsend on Plague and Yellow Fever, New York Journ. ii. 46; Emlen, N. A. Med. and Surg. Journ. v. 328; Humboldt's New Spain, ii. 765; Fever of New York in 1805, Med. Repos. ix. 213; Vaughan, Fever of Wilmington, 12, 20; Wheaton, Med. Repos. x. 335; Harris (Tucker), Barton's Journ. ii. 29; Monro, Med. Repos. iii. 136; Ramsay, *ibid.* iv. 219; Seaman, *ibid.* iv. 249; Opinion of the Medical Faculty of Baltimore on Fever of 1800, Med. Repos. iv. 353; Archer, Med. Recorder, v. 61; Bond (Thomas), Lecture, N. A. Journ. iv. 271; Deveze, 197; Brent, Med. Repos. ii. 390; Rayer, Fever of Barcelona, 43, 48; Audouard, 414; Pariset, Relat. Hist. 93; Miller (Edw.), Rep. 88, 106; O'Halloran, 118; Drake, 608; Reese (Meredith), Observations on the Epidemic of 1819, as it appeared in a part of the city of Baltimore, 46.

not difficult to be found, of vessels infected with yellow fever, and which have been rendered healthy, often rapidly, on reaching a cold climate, and passing through the ordeal of frosty weather.[1]

"In the event of a ship's company being attacked by fever," says Dr. Bryson (227), "whether from external or internal causes, which shows a disposition to become general, and to assume a malignant form, characterized by intensity of action, early yellowness of the skin, and black vomit, it will be of the greatest importance for the safety of all on board, that she should immediately quit the locality where the disease originated, and proceed, with all possible haste, to some colder region; if in the south, to the southward, and, if in the north, to the northward, avoiding the neutral ground between the trade winds. The great utility of this measure was practically tested by the Vestal in 1835, when her crew were assailed by fever at Port Royal, in Jamaica, which did not cease, although she was shifted from the inside to the keys on the outside of the harbor, nor until after she had gone far beyond the precincts of the island, and entered the twenty-seventh degree of north latitude on her way to Bermuda. The crew of the same vessel, although not the same men, after having been paid off and recommissioned, were again violently attacked by fever whilst cruizing among the windward islands of the West Indies in the latter part of 1839. Instead of running at once to the northward, she proceeded to Carlisle Bay, where she remained about a fortnight; during that time the disease evidently increased in malignancy, and carried off a considerable number of men. She was then directed to proceed to the northward, and again the disease disappeared a few days after she had crossed the tropics. The ship's company of the Vesuvius were promptly relieved of an invasion of fever by her being ordered from Sacrificios, where it was contracted, to Halifax."

These facts, observed in such divers places, and attested by so many respectable and even eminent authorities, and which ought to be, and I have little doubt are, familiar to the disbelievers in the efficacy of frost in arresting the progress of malarial fevers, taken in connection with the statements made by Humboldt, in reference to Vera Cruz, that the vomito or yellow fever seldom begins to

[1] Trotter, i. 357; Caillot, 114; Doughty, 25; Ferguson's Recol. 143; Keraudren, 18; Pym, 2d edit. 65, 127; Bryson, Report on the Climate and Principal Diseases of the African Station, 53; Barrington, Am. Journ. xii. 309; Waring, Med. Repos. iv. 1, 234; Allan, Edinb. Monthly Journ. xi. 326. See also cases of the Gen. Green, Med. Reposit. iv. 1, 234; of the U. S. Ship Hornet, Am. Journ. xii. 307.

prevail there before the average temperature of the early months reaches to twenty-four degrees of centigrade thermometer (75.2 degrees of Fahr.); that in December, January, and February, when the heat remains below that limit, and the cold is often very sharp, the disease usually disappears completely; that the latter declines sometimes very suddenly, through the effects of the north winds (los nortes), an observation also made in more southern climates;[1] that the later these continue to blow in the spring, the later the fever makes its appearance; and the sooner they commence in the autumn, the less apprehension is felt respecting its continuance;[2] all this, I say, should suffice to carry conviction to all unprejudiced minds.

Nay, more, it may be mentioned, in farther corroboration of what precedes, that the history of the Oriental Plague, whatever may have been its course in some places, and under special circumstances—as, for example, at Aleppo—whatever may be the usual order of events in Egypt and Smyrna, and however the disease may have comported itself during casual epidemics in other cities of Europe and Asia, furnishes additional illustrations of the power of cold and frost, in checking the progress of malarial complaints, among which that disease must undeniably be classed.

The epidemic of London, in 1665, was arrested in December by frost.[3] "Like everything else, too, in nature," says Sydenham, in speaking of the plague, "it has its proper periods of increase and decline; it takes birth at the period given above (when spring passes into summer), and it rises towards maturity as the year advances; with the decline of the year it declines also. Finally, the frosts of winter transform the atmosphere into a state unpropitious to its existence."[4] The epidemic of Marseilles, in 1720, ceased on the approach of winter.[5] At Erzeroum, the capital of Armenia, the disease, in 1840, broke out in June, and in 1841, in August. In both instances, it was put a stop to by the severe cold of winter.[6] The plague epidemics of Constantinople invariably commence in the summer season, and are effectually cut short by frost.[7] Volney,

[1] Catel, 8, 9; Pym, 2, 8, 216; Caillot, 106; Dariste, 31.

[2] Humboldt, 4th ed. 765.

[3] Hancock, Laws of Contagion, 91; Hodges, Loimologia, 5, 27.

[4] Vol. i. 100, ed. of Syd. Soc.

[5] Bertrand, Peste de Marseilles, 255.

[6] Report of Academy of Medicine, 31.

[7] Brayer, Neuf ans à Constantinople, ii. 77; Clot Bey, Traité de la Peste, 225; Rapport fait à l'Acad. Roy. de Méd. 330.

who, among others, has noted the fact of the cessation of the disease on the accession of winter, says, properly: "The winter destroys the plague at Constantinople, because the cold is there very severe. The summer lights it up, because the heat is damp." In Russia and Poland, the same result has been noticed. If in Egypt, and other hot latitudes, the disease usually prevails in winter—commencing in November and ceasing in June—it is because that season is warm and damp. But even there, as we gather from no less an authority than Desgenettes,[1] while the south winds, as well as hot and damp air favour, if they do not occasion its development, the north winds, and the extremes of cold or heat, put an almost complete stop to it. Much the same statements are made by Larrey,[2] Clot Bey,[3] Assalini,[4] and Pugnet.[5]

Mr. E. Robertson, who, during a long stay in Syria, paid much attention to the subject of the etiology of the plague, says that the disease in Turkey and Lower Egypt can only exist in a temperature between sixty and eighty degrees (Fahr.), "a lower or higher either modifying or utterly destroying that atmospheric constitution, or those other occult causes, giving rise to its origin and propagation."[6] Nor can the occasional continuance of the plague at Aleppo, as we learn from the admirable accounts of the disease handed down to us by Drs. Alexander and Patrick Russell,[7] through the winter, after the accession of frost, and even a fall of snow, be cited as militating against the view here maintained. Such instances of continuance were isolated and exceptional. In general, the disease has there ceased before the accession of winter—often during the months of August, September, and October—having begun to decline in July; and frequently it has ceased later on the appearance of cold. In those instances, as in 1762,[8] when different results were obtained, the cases were few in number, and it is not unreasonable to attribute them partly to the latency of the poison in the system of persons exposed to its influence previous to the change of weather, and partly to the short continuance of a temperature sufficiently

[1] Hist. Med. de l'Armée d'Orient, 248.

[2] Campagnes d'Egypt, 330.

[3] De la Peste, Observée en Orient, 266.

[4] Observations on the Disease called the Plague, &c. 42.

[5] Mém. sur les Fievres de Mauvais Caractère, &c. 204.

[6] Medical Notes on Syria, Edinburgh Journal, lxii. 331.

[7] The Natural History of Aleppo, 2d ed. iv. London, 1794; A Treatise on the Plague of Aleppo in 1760, 1761, and 1762. London, 1791.

[8] A Treatise, &c. 44.

low to destroy completely the poison, which broke out anew on the accession of heat.[1] It is true that, in the neighbouring villages, caves, and grottoes, the disease continued some time in winter, after having ceased in the city; but in these places it showed itself exclusively among the poorer classes, whose habitations, from the mode of their construction, their want of proper ventilation, the filthy condition in which they were kept, and the great heat in which their inmates indulged, may well be supposed to have retained the poison in sufficient force, if not to have been hotbeds of the pestilence, during the comparatively moderate and short winter of that country, where, according to Dr. A. Russell, the thermometer, in a series of nine years, varied in October, from 51 to 84; in November, 44 to 65; in December, 40 to 55; January, from 34 to 57; in February, from 48 to 55; in March, from 44 to 67; in April, from 56 to 82; while in May it mounted up from 67 to 92. To this it may be added that, in most places where the plague has prevailed during the winter months, as in Egypt, at Malta, Toulon, Aix, Venice, Messina, &c., that season is not characterized by severe or long-continued cold weather, and a hard bleak frost is seldom if ever experienced.

If, nevertheless, the statement of the purifying agency in question is still denied—if, while acknowledging, what no one can justifiably impugn, the truth of the events recorded, it is maintained that, in the association of the accession of frost with the cessation of fever, we are to recognize nothing but the occurrence of a fortuitous coincidence, we must admit that the frequency of that coincidence, and its manifestation in so many and diversified places, is to be viewed as a matter of the utmost astonishment, seeing that everywhere the one event is sure to follow closely on the heels of the other.

As an offset to this it has been remarked that, in the Havana, the same thing happens without frost; and that hence, when the latter occurs in any place, at the close of the epidemic season, we have no right to affirm that it produced the effect assigned to it. This cessation of fever without the aid of frost may be true. Indeed, I know full well that it is annually observed in tropical climates—not only in the Havana and other parts of Cuba, but in the West Indies generally, and on the coast of tropical America and of Africa. Nor could it be otherwise. There frost or intense

[1] A Treatise, &c. 45.

cold is a thing never heard of, and yet fever epidemics come to an end. I know, also, that it has occasionally occurred in some parts of Europe, in the United States, and even in this very city. I know full well, besides, that in some epidemic visitations, observed in both tropical and extra-tropical regions, the disease has stopped, not only before the accession of frost, or cold or cool weather, but even before the cessation of great heat. None will be disposed to deny such occurrences, who have perused attentively the history of West India epidemics, and of those that appeared in this city in 1803, in Mobile and New Orleans in 1848, as described by Drs. Caldwell, Nott, and Fenner, and in Leghorn, in 1804, as related by Palloni. But these facts, true as they doubtless are, do not in the least invalidate the reality of the power which I have, in common with so many others, ascribed to cold and frost; for, from the circumstance that yellow fever has sometimes stopped in this country, and usually ceases completely, or in great measure, in tropical regions, without the aid of frost, and sometimes before a cessation of high atmospheric heat, we cannot argue that frost, or severe cold, when occurring before the fever has been arrested in its epidemic course, will not produce that effect. This cessation often is too instantaneous, and is, besides, too constantly noticed, after its occurrence, to be ascribed to any other agency. The two results are not incompatible. While, therefore, we maintain that the effect arises most usually from the last-mentioned cause, we admit, what experience has sufficiently demonstrated, that the same beneficial change is also produced through other agencies—heavy rains, violent storms, heavy winds, especially from the north, desiccating and long-continued heat and droughts, and not unfrequently from the want of subjects susceptible to the morbific impression of the poison; and that, at times, it is brought about by a change in the epidemic meteoration, the evolution of ozone in the atmosphere, or some other purifying influence, the nature of which has so far eluded our researches; a change which, as Dr. Smith[1] remarks, is, in effect, equivalent to the reduction of the temperature to 32° of Fahrenheit. The same effects have been noticed in other forms of malarial fevers; and every one is familiar with the fact that, in Egypt, where, for reasons mentioned, the plague prevails during the winter months, it stops in June, under the empire of the same

[1] On Epidemics, 174.

parching heat which in that country arrests the progress of animal putrefaction. But, I repeat, were these occurrences more frequent than we know them really to be, they could not serve to counterbalance and overthrow all that has been said in support of the agency ascribed to frost and severe cold, in arresting the progress of yellow and other malarial fevers.

Very different are the results obtained in regard to pneumonia. For reasons plain enough to those who bear in mind what is known of the causes, habitat, and usual periods of prevalence of that disease, the occurrence of frost, or the accession of cold weather has never put a stop to it. So far from it, while such a favourable change is thus experienced, as regards the prevalence of fevers through the means in question, it is found that pneumonic inflammations, instead of disappearing like the former, continue to show themselves as though nothing had occurred. Indeed, whatever may be the tendency to the disease in any locality—whether the number of cases be very large or small, and whether they occur in summer or autumn—the attacks multiply on the accession of the very influences which had put a stop to the other complaint; while, after prevailing during the winter and spring, and taking the place, as it were, during those seasons, of that class of fevers to which attention has been all along called, their reign is, in its turn, more or less effectually arrested by the return of the atmospheric conditions under the empire of which those fevers are developed. Let pneumonia prevail on board of a ship in some warm latitude—a thing which has occasionally occurred—and it may be doubted whether the commander would gain much by steering north and reaching the banks of Newfoundland. The one disease, pneumonia, belongs more specially, as we have seen, to winter and early spring, and, if not arrested completely, is at least greatly diminished, in point of frequency, by the return of heat. The other is a disease of summer and autumn, and is diminished by cool weather and arrested by frost. Nothing could be more different. It affords a strong illustration of the correctness of a remark long ago made by Hippocrates, that the diseases of winter are put a stop to by summer, and those of summer by winter. They are, to a certain extent, antipodal or antagonistic. At any rate, the occurrence above stated—the cessation of fever through the instrumentality of frost, and the continuance or increase of pneumonia after the accession of the latter, cannot be regarded otherwise than as an event the very

opposite of that which might be anticipated were the diseases offsprings of one and the same cause, and were the latter, pneumonia, really and substantially nothing more than a peculiar form of remittent and intermittent fever. To affirm this, in the face of the facts just mentioned, it will be necessary to put the syllogism thus: Autumnal fevers are arrested by severe cold and frost; pneumonia is unaffected by such atmospheric changes, or even becomes more rife after their occurrence; *ergo*, autumnal fevers and pneumonia are identical, or the latter is only a peculiar form of the other. Again: Fevers appear and prevail in hot weather; the reign of pneumonia ceases on the accession of such weather; *ergo*, the two diseases are the same; and he who ventures to deny their identity, gives proof of being sadly behind the age, and ought, without loss of time, to imbue himself with the principles of the Baconian philosophy.

Writers who uphold the opinion under examination will not, I trust, strive to strengthen the position they have assumed, by insisting on the circumstance that as pneumonia—which, they say, is really and substantially nothing more than a peculiar form of autumnal fevers—continues to appear after the accession of cold and frost has put a stop to the ordinary forms of the latter, etiologists, who maintain that the cause of febrile diseases generally is destroyed by a reduction of temperature below the freezing point, on the ground that such diseases disappear after that atmospherical change, are open to the charge of hazarding an assumption and reaching a conclusion unwarranted by the premises; inasmuch as all that may justly be deduced from the phenomena observed is, that some forms of periodic fever, not all, are arrested by the advent of cold; and that the cause has been merely so modified as to cease to produce a certain form of the disease, while it continues to possess its original power of occasioning certain other forms. The force of this objection few will feel disposed to admit; for, before it can be used with success in the settlement of the question at issue, it would be first necessary to demonstrate a real identity or close alliance, in a pathological or symptomatological point of view, between pure pneumonia and malarial fevers. But this, as I need scarcely remark, has not as yet been and never can be done. I shall have occasion to recur to the subject more in detail hereafter.

Of necessity, if periodic fevers are arrested by frost and cold, this salutary effect is due to the destruction, by this reduction of

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temperature, of the cause of the disease, or to the sudden removal of one or more of the elements which enter into the composition of that cause. Of the nature of the latter I can entertain no doubt. In common with many a clever man, from time immemorial to the present day, I take it to be nothing more nor less than malarious exhalations—poisonous particles, floating in the atmosphere, and arising from peculiar changes in various organic substanees in a state of decomposition. But, exclaim some of the advocates of the opinion under examination, all that has been said about this destruction of the cause of febrile affections—admitting, for the sake of argument, that these do cease on the accession of frost—is an assumption; for we cannot show that a thing is destroyed, the existence of which has not been proved. Far be it from the author of this volume to insist on the possibility of frost destroying a thing which has no existence; and if it can ever be proved that the morbific cause in question must be placed in that category, I shall certainly feel no disposition to say anything more about it. But, has this been done? So far from it, we look in vain, in what has been written on the subject, for anything calculated to disprove the existence and morbid agency of malaria, and yet this ought to have been well established before it could justly be adduced against a belief in the destructive power ascribed to frost, and the mode in which it is stated to exercise its salutary effects.

For my part, with due deference to the judgment and learning of those who entertain an adverse opinion, I have no hesitation in saying that the more I examine the subject, the more I am convinced that those who ignore the existence of malarious exhalations, and deny their morbific agency, labour under a great error, and eontend for what they cannot prove. Nay, more—I am convinced that autumnal and endemie fevers, of genuine character, are never due to *any other* cause than such exhalations. Believing this, and holding, at the same time, what every one knows to be true, that pneumonie inflammations are due to very different agencies, it may not be improper, in order farther to show the fallacy of the views of those who insist on the close connection between those diseases, to dwell on some of the leading facts and arguments on which a belief in the existence of such exhalations, and in the peculiar and specific effects ascribed to them, is founded. I am aware that, as regards several of the advocates of the connection mentioned, the task is unnecessary, inasmuch as they admit the existence and morbid

agency of malaria; and that, while regarding periodic or autumnal fevers, of all grades and types, as caused by the latter, they do not hesitate to recognize in pneumonia a similarity of origin; some applying the doctrine to all manifestations of that disease; others, perhaps the greater number, restricting it to certain cases, characterized by periodical changes, and cured by remedies found successful in intermittent fevers. By others, however, a different opinion is entertained on the subject; and while some among them content themselves with remaining in doubt as to the propriety of admitting the existence of a specific febrile poison, others offer a decided and uncompromising opposition to the malarial origin of fever, holding all that has been said in its support to be a mere effect of a disordered imagination, and an hypothesis unworthy of this age of progress. The importance of the subject in its bearing upon the question more particularly before us, will be my excuse for entering upon the examination of it in detail. But I must reserve it for a separate chapter.

CHAPTER II.

EXISTENCE AND MORBID AGENCY OF MALARIA.

In the preceding chapter, attention was called to various facts militating against the opinion entertained, both in olden and modern times, by some medical writers, respecting a supposed close connection, pathological and etiological, between pneumonia and periodic fevers. In the prosecution of the inquiry, I was led to a consideration of some circumstances connected with the causation of the latter diseases, and, in so doing, touched frequently upon the subject of malaria, which was spoken of as the long admitted efficient agent in their production. Of the antiquity of this opinion, and of its very general adoption at the present day, few need be told.

Hippocrates was of the opinion that diseases in general may be said to arise either from the food we eat, or the air we breathe. When, therefore, a disease seized on a multitude of persons of different ages, sexes, and habits, he inferred that it must arise from the latter cause. Animals, he says, are under the influence of the air. Hence, in all probability, the source of diseases must not be sought elsewhere whenever it enters the body, either in excess, in deficient quantity, too suddenly, or contaminated with morbific miasmata. Of epidemic fevers, the cause resides in the air. If, under such circumstances, all animals are not affected at the same time, and only one species suffers from the disease, it is because all things, bodies, natures, aliments, are not alike, and what is proper or improper for one species, is not necessarily equally so for other species. When, therefore, the air is infected with miasms that are enemies of human nature, men are sick; when, on the contrary, the air becomes injurious to some other species of animals, the latter alone is affected. (*De Flatibus*, sects. 5, 6.) In his treatise on Airs, Waters, and Places, he attributes the greater part of the injurious effects of stagnant streams, and marshy situation, to drinking of the water therein found (sects. 7, 8, 10, 15). But there is

much in that famous treatise which—especially when taken in connection with what the author remarks in his *Discourse on Winds*, just quoted, *i. e.* that with all we eat or drink, air finds entrance into the body, and injures it when tainted with miasma—leads us to infer that, in his opinion, such waters do not affect the human system injuriously, simply by being ingested, but sometimes also by the miasma contained in or evolved from it. But whether, by the term miasma, he understood the marsh or terrestrial effluvia of modern times, is a question decided in the negative by some, and which others, as Dr. Adams,[1] consider as impossible to be determined. For my part, I am inclined to believe—apart from the references already made—that the great Greek observer, though not pointing out distinctly the effects of marsh effluvia in engendering the periodic fevers he so well describes, and having formed no just conception of their nature and precise sources, recognized the existence of miasma as a separate morbific agent; for he commences the meteorological year from the decline of the summer preceding the year in which the epidemic he is about to describe occurs, and speaks of diseases arising from a certain succession of seasons. Now, as a determinate series of intemperies of several seasons cannot produce a special and well-defined effect on the system, otherwise than through the medium of exhalations of some sort from the earth, which are by them occasioned, we cannot greatly err in inferring that he had such exhalations in his mind when using the term. Hence it is, Hippocrates applies to the cause of epidemics the name of *something divine*—an expression which Galen, in his commentaries on the epidemics, regards as meaning alterations and affections of the air arising from the influence of the stars. Nor is it less probable he included under that same name an invisible fluid floating in the atmosphere, and occasioning in it the alteration and affection above alluded to; for he advocated the opinion of Pythagoras and Heraclitus, that all that exists under a sensible form arises from invisible corpuscles. But, however this may be, it is not to be forgotten that neither Hippocrates, nor his contemporaries, were ignorant of the fact that the atmosphere in the vicinity of marshes and large rivers in warm climates is unwholesome to the inhabitants;[2] that in his aphorisms he refers diseases to the changes of seasons and variations of temperature occurring during these; and that, in

[1] Transl. i. 349; Syd. Soc. Ed.

[2] Treatise on Regimen, ii. 2.

the third book of his Epidemics, he ascribes the fatal or pestilential disease observed by him, on a particular occasion, to the preternatural and unwholesome state of the atmosphere.

Indeed, that a knowledge of the deleterious effects of certain conditions of the air in producing fever existed among the contemporaries or immediate successors of Hippocrates; that they knew full well that such an unwholesome air was the result of something extraneous, more particularly exhaling from marshy surfaces; and that they were fully aware of the advantage and necessity of changing the physical conditions of these, may be inferred from many of the allegorical expressions in which they were so wont to indulge, as well as from some passages in the writings of ancient philosophers and historians. In accordance with the custom just alluded to, of resorting to allegorical expressions to convey important truths and salutary admonitions, and to communicate useful knowledge, the ancient Greeks often employed the words monster, serpent, venomous beasts, as emblematic of marshes, and of the poisonous and fatal air issuing from them. It is in this sense, doubtless, as was long ago surmised, that we are to understand the fictions concerning the hydra, the chersydra, and the monster python, slain by Apollo; derived, as those words are, the one from the Greek root, meaning putrefaction or corruption; the others from the word water. Thus it is they personified the many-headed hydra under the emblem of a huge water-snake, whose poisonous breath infected the air, and imparted disease to animals that breathed it. Nor are we less justified in inferring that the terms hydra and chersydra were allegorically employed to designate the two and opposite conditions of marshes—the one, in which these are covered with water, the other in which they are left bare—hydra representing the snake concealed in the bosom of the stagnant and miry fluid, and chersydra meaning the desiccation, in hot seasons and regions, of those localities, when they give issue to an air more virulent and poisonous than that connected with their overflow. Every one recollects the story of Hydra, the celebrated monster which infested the neighborhood of the lake of Lerna, in the Peloponnesus, with its numberless heads—one hundred, according to Diodorus Siculus; fifty, agreeably to Simonides; and nine, if we may credit Apollodorus—and presenting this remarkable peculiarity, that the central head was immortal; and, as regards the others, as one was cut off, two immediately grew up, if the wound was not cauterized by fire. It was one of the labours of Hercules to destroy this dreadful monster; an ope-

ration he easily effected with the assistance of Iolaus, who applied burning iron to the wound as soon as each head was cut off. The allegory appears so plain as to induce many modern writers to adopt the views above expressed, and to consider this fable as a symbolical representation of the clearing and draining of the Peloponnesus by the first authors of civilization. It was so regarded by some of the ancient writers themselves; for Antipater, the stoic, speaks of the "exhalations from the damp or wet earth rising up like a winding stream with a burning force, and, after having been heated, darting down again in the way of a deadly serpent, poisoning and infecting everything by means of its corrupting agency." "Not without reason," he adds, "did Heraclitus give the name of arrow of Apollo to the rays of the sun, for they beget corruption in the dampness of the soil. Hercules, the greatest subduer of the foggy atmosphere in times past, was placed among the gods, for having destroyed the hydra; in other words, for having reclaimed the marshy desert."

Such being the case, we may infer that the early Greeks, though doubtless far from possessing correct notions as to the nature of malaria, as understood at the present day, or having an insight into its laws, were perfectly conversant with the deleterious effects of marshy surfaces; and aware, also, that the diseases incidental to such localities, are due not to changes in the sensible qualities of the atmosphere, but to a peculiar condition of the latter arising from the admixture of a poisonous element issuing from the stagnant water of marshes, and rendered still more baneful by the partial desiccation of the miry surface. The history of Empedocles, and more particularly of the means he employed to arrest the pestilence by which the city of Selinonte was ravaged, to which I shall have occasion to revert in a future chapter, would seem to prove, beyond doubt, that he at least was fully aware of the agency of noxious exhalations in the production of febrile diseases.

At a period less remote from our own, the Romans, without having a very clear idea of the manner in which malaria is produced, expressed opinions and offered explanations of facts, which would do no discredit to modern etiologists; for though differing as to what the thing exactly was which exhaled from marshes (considered as a philosophical rather than practical question), contaminated the atmosphere, and caused sickness, and though indulging in a variety of whimsical opinions on the subject, enough is found in their writings to show that they had recognized the unhealthiness

of marshes and marshy surfaces, and ascribed the effect to something beyond modifications in the sensible qualities of the atmosphere. Lucretius, who, though only a poet, may reasonably be supposed to have reflected, in his *Rerum Naturæ*, the scientific views of the professional men of his time, observes (vers. 1,100), that the cause of epidemic and pestilential diseases may be referred to one or other of two sources: first, to the atmosphere, through the agency of germs wafted to the sickly place by the winds, in the same way that these impel clouds from place to place; or, secondly, to the earth itself, when the latter, through means of long-continued rains and strong atmospheric intemperies, covered over with water and heated by the rays of the sun, undergoes, together with the remains of organic substances spread over its surface, the putrefactive process, and thereby gives vent to miasms and germs analogous to those before mentioned as wafted from other places. Virgil sings of the emptiness of the city of Acerræ, whose population had been thinned by the sluggish course of the River Clanius—*et vacuis Clanius non æquus Acerris.* Varro ascribed the mischief to swarms of insects. "It is worthy of remark in marshy places," he says, "that, as they dry up, there are produced certain very small insects, too minute for observation by the eye; which, being taken into the body by the mouth and nostrils, are the cause of difficult diseases."[1]

Columella writes that "a marsh ought not to be in the neighbourhood of buildings, nor military way, because, when acted upon by heat, it ejects a baleful poison, and engenders animals armed with troublesome stings, which settle upon us in the thickest swarms. Then, too, it emits the venomous hosts of water-snakes and serpents, freed from their winter's slime, mud, and fermentative colluvies; and from these there arise frequently obscure diseases, whose causes have not been investigated, even by physicians."[2] When we come to inquire what these obscure diseases were, we find that they consisted of the several varieties of autumnal fevers.

Palladius is of opinion that "a marsh is to be avoided upon every principle, especially on the south or west, or if it usually dries up in summer, because it generates pestilence and hostile animals."[3] Vitruvius says that "the vicinity of marshes ought to be shunned, because when the morning breezes reach the house, with the rising

[1] De Re Rustica, lib. i. cap. 12.
[2] De Re Rustica, lib. i. cap. 5.
[3] De Re Rustica, lib. i. tit. 7.

sun, they bring with them mists and exhalations tainted with the poison of the marshy brood. And this mixture of venom with fog is conveyed by the winds to the bodies of the inhabitants, and renders the place pestilential."[1]

Diodorus Siculus (xiv.) states that, at the siege of Syracuse, the Carthaginians, being encamped in the immediate vicinity of an infectious marsh, and exposed to the thick and heavy vapours issuing therefrom, and being moreover accumulated on a low and humid locality, a pestilential fever broke out among them, and gave rise to a large mortality. To the same cause he attributes the plague of Athens (xii. 58). Galen attributes the origin of epidemics to the state of the atmosphere, at least in a great measure, but he also maintains that the nature of the country may contribute to the same effect; as, for example, its vicinity to a gulf like the Charonean, from which miasmata are exhaled. From these the air is tainted, and diseases are produced. Thus, in many passages of his *Commentaries on the Epidemics of Hippocrates*, he states that such diseases are to be referred to the first of these causes—the condition of the country in which they prevail; and in another work, he openly and explicitly states that, for the most part, they are derived from the atmosphere being tainted with putrid exhalations. He ascribed a large share of agency in the production of the impurity of the air to intense heat and the decomposition of organic substances, observing that the putrefaction of dead bodies left unburnt on a field of battle, in hot weather, is a fruitful cause of pestilence, under which name wide-spreading fevers of various grades were then included, and adds that, in other cases, the impurity is occasioned by the exhalations of certain marshes or lakes in the summer season.[2]

Judging from the little he says of epidemics, Celsus may be regarded as having entertained opinions similar to those of Hippocrates.[3]

Not very different is the opinion of Paulus Ægineta: "The nature of the country will also often occasion common diseases, either from its lying adjacent to marshes, or to some deep pit which emits a deleterious and pernicious exhalation."[4]

According to Hally Abbas (*Theor.* v. 2), the principal causes of a pestilential state of the atmosphere are, the nature of the country

[1] Archit. lib. i. cap. iv.

[2] De Febribus Diff. lib. i. cap. 4.

[3] De Re Medicina, i. 47; Paris edit.

[4] The Seven Books of Paulus Ægineta; Adams's transl. i. 273.

and the season of the year. The former cause produces its effects through means of the effluvia arising from corrupted fruits, pot herbs, &c., or the miasmata from marshes, cloacæ or dead bodies, whether of men or cattle. Avicenna (iv. 1, 4) differed little from Galen, attributing fevers to a humid and warm state of the atmosphere, the stagnant air of caverns, the miasmata of lakes and marshes, and the effluvia from dead bodies. Avenzoar also attributes epidemic diseases to a humid and warm state of the atmosphere, effluvia from dead bodies, stagnant air, the miasmata from stagnant and corrupted waters, and unwholesome food (iii. 1, 3).[1]

The summer and autumnal fevers of Rome and the adjoining territory attracted the notice of and are often alluded to by Livy, Strabo, Dionysius, Dio Cassius, and others; and sufficient was said on the subject to show that already at a very early period the cause, though not thoroughly understood, was not viewed as similar to those giving rise to ordinary diseases, but very generally ascribed to an impure state of the atmosphere, the effect often of the overflow of the Tiber, and of other kindred occurrences. The opinion of the production of fevers through the agency of exhalations gave way to the fantasies of the Middle Ages—occult causes, conjunction of the planets, &c. Whatever was left of it was almost completely absorbed or chased away by the doctrine of the *contagium*, which, in the hands of its talented originator, Fracastorius, and of his many followers, was made to explain almost every epidemic disease. Though at all times experiencing opposition from writers, who, like Montanus and Valeriola, adhered more or less strictly to the views of their ancient masters, the doctrine of contagion may be said to have remained in the ascendant till the days of Fernelius,[2] to whom credit is due for having been the first, after the renewal of letters, to show the influence of contaminated air in the production of epidemics, and, as M. Rochoux has well remarked (p. 125), laid down the doctrine of miasmatic infection in a way which leaves scarcely anything to desire. Niccola Massa, Alphani, Porteus, Paré, Pisanelli, advocated opinions relative to the causes of autumnal fevers not very different from those generally entertained at the present day.

Indeed, almost all the Galenics, down to Sennertus, comprehended

[1] See Adams's Commentaries on Paulus Ægineta, i. 276, &c.

[2] Universa Medicina, &c., De abditis rerum causis, lib. ii. 497, 1552.

the universal noxiousness of muddy and swampy places, under the naked words evaporation, exhalation, and emanation, and ascribed it to certain poisonous qualities of the air. Fracastorius himself states that, in 1528, a pestilential fever prevailed extensively in Italy, and was caused by an extraordinary overflow of the Po, and consequent formation of numerous marshes.[1] Prosper Alpinus, who practised in Egypt from 1580 to 1584, ascribed the plague of that country to morbid exhalations.' In reference to the pestilential fever of Alexandria, he remarks that opinions varied as to its cause. By some it was attributed to putrid exhalations arising from the lake of Mareotis, and conveyed to the city by the winds. Other physicians referred them to putrid and poisonous exhalations mingling with the air, and arising from putrid and bad water contained in reservoirs under the city, which were filled during the overflowing of the Nile, in order to afford a supply to the inhabitants throughout the year.[2] The agency of marshes, pools, pits for macerating hemp, unburied bodies of men and animals, and similar sources of contamination of the atmosphere, "are familiarly mentioned by a writer who calls himself Troilo Lancetta, and who published at Venice, on the 16th of June, 1632, a short treatise on a pestilence then prevailing at Venice, and on the means of arresting its progress."[3] Ramazzini pointed out very accurately the causes of the epidemic fevers which for several years prevailed in and about Modena toward the close of the seventeenth century, attributing them to the action of an atmosphere tainted with many acid and earthy exhalations rising from a muddy, and, as it were, a fermented soil.[4] Bontius, who practised for several years at Batavia, prior to 1631, describing the state of the air, says: "It is rendered insalubrious, not only from its heat and moisture—the promoters of putridity—but also by the stagnant and marshy places with which the country abounds; and the wind, blowing from the mountains, bring to the city from these marshes, dense, fetid, and poisonous exhalations, which corrupt the air."[5] Sylvius (De le Boe) traced an epidemic which broke out at Leyden in his time to the stagnant

[1] De Morbis Contagiosis, lib. ii. cap. vii. in Opera Omnia, 100.

[2] De Medicina Ægyptiorum, lib. i. cap. 14; 25–6. Paris, 1645.

[3] Ed. Med. J. lxxviii. 231.

[4] De constitutione anni 1690, de Epidemiâ quæ Munitensis agris et Vicinarum regionem, &c. Modena, 1691, 4to.

[5] De Medicina Indorum, Dialogus, i. 13.

and corrupted waters from which exhaled noxious vapours, which remain in the lower part of the atmosphere.[1] Hoffman, when treating of the evil results of a moist atmosphere, says: "The cause of all these accidents is justly and properly traced to the atmosphere, rendered sluggish, heavy, and destitute of elasticity, by marsh effluvia."

Baglivi, whose work on the practice of physic was written about the close of the seventeenth century, remarks on this subject: "The Roman air is likewise foul and unwholesome—not in all places, indeed, but those chiefly where houses are wanting, and the air is slow and unmoved; and, above all, in such places as lie upon the Tiber, or, like valleys, are hedged in by mountains, or are exposed to the exhalations that rise from old ruinous walls, vaults, and the rubbish of the ancient edifices. Hence, it is manifest that the quarter of the Circus Maximus, lying between the Palatine and Aventine, the Tiber and Ostian Gate, is very unwholesome and pernicious."[2]

About the same period, Chirac, in describing the epidemic of malignant or yellow fever which prevailed at Rochefort in 1694, and was then designated under the name of *plague*, says of that place, that it is situated on the River Charente, and is sheltered from the north wind by a high hill and the remains of a thick wood. On the east side is situated a large meadow, which is almost annually overflowed by the river, and thereby converted into marshes filled with muddy and stinking water. These marshes dry up during the summer, and impart to the air of the port a smell of burnt powder, especially towards evening, when the dew begins to fall. To these exhalations—which he regards as always extremely dangerous—Chirac attributed the fever in question. This inference he considered natural, because he had before his eyes the results of observations made annually in all paludal countries, "whose inhabitants are scourged by malignant fevers, which there break out, almost every summer, when the marshes are dried up, and the air becomes surcharged with the indigestible and offensive sulphurs which exhale from the mud.[3] By Porzio and others attention was called, about the same time or not long after, to the Hungarian fever. Finally, Lancisi, collecting all the facts and information already possessed, and adding many he himself had amassed during

[1] Oratio de affectûs epidemici Leidensis causis naturalibus dicta, 1670. 12mo.
[2] Opera Omnia: Venitiis, 1754, 81.
[3] Traité des Fièvres Malignes, &c. i. 31, 141, 147.

a period of observation of thirty years, made them illustrate the etiology of epidemic and endemic diseases in general, which he ascribes, as every one knows, to marsh and other exhalations.[1]

From that day to the present, the existence of a poisonous matter floating in the atmosphere of febriferous localities, and its special agency in the production of autumnal fevers, have been admitted by professional men of all countries. The sources whence it is derived, the atmospheric conditions which favour, retard, or prevent its development, and the laws by which it is governed, have been investigated; its geographical and altitudinal limits have been traced; the baneful influence it exercises, the various characters it assumes—as shown by the diversified forms of fever it produces—the modifications it imparts to other complaints, and the particular mode of treatment it renders necessary, have been pointed out in many didactic works, and in more numerous publications of less pretension.

The agency of malaria not universally admitted.—It is scarcely necessary to remark, that however evidently well founded the inference respecting this agency has appeared to professional observers in all parts of the world; however universally, indeed, the doctrine which ascribes autumnal or periodic fevers, of all grades and types, to the morbific influence of malarial exhalations, has been recognized, during a long succession of years, as placed beyond the possibility of doubt or disputation, its advocates have not been left in undisturbed possession of the field. Doubts as to its correctness have been expressed; objections have been raised; and, by more than one writer, the very existence of malaria, or its agency in the production of autumnal fever, has been denied. And why should it be otherwise? The reader must be aware that it would be difficult to discover an admitted fact in practice, or a correct or plausible point in theory, whether on pathology or etiology, which has not, at some time, or in some place, encountered greater or less opposition, or whose correctness has not been flatly denied. Who does not know that the doctrine of the circulation met for years with the most strenuous opposition, and could not obtain a footing in some of the most renowned schools of Europe? The remedial effects of antimony, of mercury, of the lancet, and other therapeutic

[1] De Noxiis Paludum Effluviis. Dissertatio de Nativis, deque adventitiis Romani cœli qualitatibus, 16.

agents, have been violently opposed, and given rise to wars which, though less imaginary, appear almost as ludicrous to us as those which, as we are told, raged between the big endians and the little endians. The specificity of morbid action has been scoffed at, and the hypothesis of the unity of all diseases extolled, even by the clearest intellects. The utility, first of inoculation, and next of vaccination, has encountered more than one virulent opponent, and the danger of the latter process has been insisted upon in volumes of large dimensions. Professional men are found in this nineteenth century, who, discarding the results of the accumulated experience of a hundred successive generations, embrace the absurdities of homœopathy, and seriously believe in the wonderful effects of a decillionth part of a grain of silex or charcoal. Hydropathy enumerates warm partisans among physicians in Europe and this country; and surely, if such has been and continues to be the case in relation to the several subjects mentioned, we cannot be surprised at finding the question of the existence or morbid agency of malaria sharing the same fate. Hence, in Italy, Giannini,[1] Folchi,[2] Santarelli,[3] and Mienzi,[4] have exerted themselves to controvert the long-received views on the subject. In France, the same opposition has been made by Lafont-Gouzy,[5] Ramel,[6] and Reveillé Parise.[7]

In England, Armstrong,[8] Hopkins,[9] Calvert, Pritchett,[10] Sir James Murray,[11] and Dundas,[12] have inscribed themselves among the opponents of malaria. In this country, too, the existence of this agent has been denied by Rumph,[13] Strobbart,[14] Jones,[15] Lee,[16] Gay-

[1] Della Natura della febbri, cap. 2, vol. i. 110, 2d ed.; French translation of d. i. 229.

[2] Sull' Origine delle Intermittenti di Roma e sua Campania. Roma, 1828. See N. A. J. v. for a translation of this Essay.

[3] Ricerche Interno alla causa delle Febbre Perniciosa dominento nello Stato Romano, 32.

[4] Sopra eegenesi delle febbri Interm. Roma, 1844.

[5] Caractères Propres, Préservatifs et Remèdes des Contagions, 1822. *See* Monfalcon, 47, note.

[6] De l'Influence des Marais et des Etangs sur le Santé des Hommes. Paris, 1802. Monfalcon, 527.

[7] Journal Général de Médecine, xcvii. 105.

[8] The Influence of Climate on the Human Constitution, 33.

[9] London and Edinb. Philos. Magazine, 3d series, No. 86.

[10] Some Account of the African Remittent Fever, 109.

[11] Dublin Medical Press, Nov. 27, 1844.

[12] Sketches of Brazil, 154.

[13] Some Thoughts on Malaria, and Doubts as to its Existence as a Source of Disease. Charleston J. iii. 37.

[14] Thoughts on Malaria. Charleston J. vi. 661.

[15] Boston J. ii. 376.

[16] Forry, Climates of the U. S. 109.

ley,[1] Bell,[2] Merrill,[3] and a few others; while everywhere writers are encountered, who, though not openly striving to disprove the special agency of morbid exhalations, say nothing about them, and content themselves with looking to other causes to account for the occurrence of fever.

Objections made to the agency of malaria, various.—But while all these various writers unite in rejecting or ignoring the existence of malaria, they differ widely as regards the substitute they propose. The one refers all the mischief to the action of heat on the system. A second considers atmospheric or terrestrial humidity as the efficient agent. A third explains all the phenomena by means of atmospheric vicissitudes—heat by day, cold by night. Then again, we are told that the true cause of periodic fevers must be sought in a low dew-point; or in the action of fogs or of visible dews, "the phenomena of which are, in fact, all the pretended laws of miasma;" or in the absence or deficiency of atmospheric electricity, or in the presence of some particular and well-known gas. There are not even wanting those who think that unnutritious food exercises a more powerful agency in the production of periodic fever than any other cause. On all these several and diversified hypotheses much has been written. Facts, and, when possible, experiments have been appealed to; learning and ingenuity have often been displayed; but from all I have read and seen on the subject, I am inclined to the opinion that the opponents of malaria have left the question precisely where they found it, and that, so far from adducing anything calculated to disprove the existence of that poison, or establishing the correctness of their own views, they have done much to involve the whole subject in inextricable confusion, by constantly confounding the predisposing and exciting influence with the efficient cause of those diseases, and raising the former to the dignity of the latter. Convinced of this, and of the existence of malaria as a distinct morbific agent; regarding it as the special cause of autumnal fevers, and as completely independent of and distinct from those influences which give rise to pneumonic inflam-

[1] Am. Med. J. N. S. xvii. 53.

[2] On Miasm as an alleged Cause of Fever. Philad. J. of Medical and Physical Sc. ii. N. S. 274.

[3] Address to the Memphis Med. Soc. on the Sanitary Condition of that City. Memphis Med. Recorder, i. 99.

mation, and, as a consequence, being persuaded that the reality of this agency, if satisfactorily established, must lend a powerful aid in the refutation of the views under examination, I propose in the present chapter, and before proceeding farther in the matter under consideration, to examine somewhat in detail the leading facts and arguments on which the malarial doctrine rests, and to note a few of the objections that have been raised against it.

The appearance of fever where there are no marshes does not disprove the agency of malaria.—By those who reject this doctrine, it is not unfrequently urged in a tone of confident triumph that its advocates ascribe to marsh miasma those very fevers which we meet with in their most malignant forms in situations where there are no marshes existing; and that this is pertinaciously followed up, although contradicted by every day's experience. Hence, they add, if fevers arise without marshes, there can be no propriety in referring them to the agency of these, when they happen to exist in sickly localities, for the sickness would in all probability have arisen had they not been situated there. All this at first sight appears plausible enough, and may prove perfectly satisfactory to those who are not well posted up on questions of the kind; but, on examination, the objection will be found to rest on very insecure foundation. For when we come to inquire by whom periodic fevers are referred exclusively to the effluvia of marshes, and especially by whom the opinion is obstinately maintained, we easily discover that nothing of the kind is insisted upon by any physician of respectable authority; by no one, indeed, who has directed his thoughts seriously to the etiology of febrile complaints. Doubtless, a large majority of medical writers and inquirers maintain that certain forms of fever are the products of marsh exhalations properly so called. Doubtless, also, other forms of the same disease, including the malignant, are as generally regarded as due to the action of exhalations of some sort. But no one, or at least few, among those whose opinions are worth recording, think nowadays, or have thought for years back, of viewing the existence of a marsh as a *sine qua non*, and of pertinaciously affirming that febrile miasmata can only be evolved from a paludal soil. Those, therefore, who raise an outcry against the absurdity of such an opinion, and tax their opponents with generally entertaining it; those especially, who, like Dr. Dundas, and others of the same school, write long dissertations

with the view to set the world right on the subject, lose much valuable time, which might be more profitably employed in making themselves well acquainted with the true state of the question—a point about which some of them appear to be lamentably deficient—than in refuting opinions which, if ever entertained, have long since been abandoned.

That such is really the case, that the opinion which ascribes all fevers to the exhalations of marshes solely, is not entertained, admits of no doubt. Even Dr. Bancroft, whose work appeared some forty years ago, and would almost seem, like some others on the same subject, to have been written with a view to mystify the reader on some parts of its contents, continually confounding together, as he does, several distinct fevers, and applying to them the one name of yellow fever; even Dr. Bancroft, I say, takes pains to explain that in joining the epithet marsh or marshy (which he almost invariably does) to the term miasmata, exhalation, effluvia, &c., and in considering them as a cause of fever, he does not mean to intimate that such miasmata, &c., are emitted solely by marshes, it being certain that they frequently arise from soils in a different state, but only to designate the quality of those vapours which are eminently the product of marshy ground. Other writers have pointed out the impropriety of viewing malaria as the production of marshy surfaces exclusively, as it arises often in arid places destitute of swampy surfaces.[1] Lancisi himself, the systematizer of the knowledge of the times on malarial effluvia, the discoverer of important facts, and the able expounder of etiological observations, who is often referred to, but seldom read, was very far from believing that intermittent and remittent fevers, which he knew full well were only modifications of each other, proceed from the emanations of marshy grounds alone. On this subject he expresses himself very decidedly.[2] He had observed, and he records, that from the soil covered by the ruins of houses, temples, and public buildings, from grounds rendered damp and filthy by the obstructions of drains and sewers, by the overflowing of the Tiber, &c., fever had arisen and prevailed extensively. The writings of Chervin,[3] Boudin,[4] Nepple,[5] Maillot,[6] Segond,[7]

[1] J. Johnson on Tr. Clim. 24; Bryson, 196.
[2] De Noxiis Paludum Effluvius, Oper. i. 73.
[3] De l'Identité des Fièvres, d'Origine Paludéennes.
[4] Géographie Médicale.
[5] Traité des Fièvres Interm. Simples et Pernicieuses.
[6] Traité des Fièvres ou Irritations Céreb. Spin. Interm.
[7] Mém. sur les Fièvres de Cayenne.

Faure,[1] &c., which are usually referred to in support of the strictly paludal origin of fevers generally, will show that, even in the opinion of these authors, malarial exhalations of various degrees of virulence may, and do often proceed from surfaces presenting characters very different from those appertaining to ordinary marshes. Indeed, at the present day, this existence of malarial exhalations, and their efficiency in the production of fever, independently of the presence of marshes, properly so called, and their elimination from sources of various nature, and differing much in external appearance, is almost universally admitted—quite so, I think, by all who have taken pains to investigate the subject in all its bearings; for, while they are firmly convinced of the reality of the morbific agency of such effluvia, they know that fevers prevail sometimes even in arid places with want of surface water, where the soil is rocky, or sandy, parched, and deficient in vegetation, and where, in a word, circumstances generally are, in appearance at least, unfavourable to the decay of organic matter. On this subject, the facts recorded by Ferguson, J. Davy, Craigie, Brown, Currie, Humboldt, and others, can leave no doubt.[2] Nay more, it is almost as generally acknowledged, that the malignant forms of such diseases are never produced by the effluvia of genuine marshes, but are the products of other miasmal sources; while, on the contrary, fevers known to arise from marsh exhalations, are never produced by the effluvia which occasion the other forms of the disease. Hence, when ordinary or malignant autumnal fevers occur in places where no marshes properly so called exist, it is of no avail to cite the absence of these as an evidence of erroneous conclusions, far less of absurdity, on the part of those who attribute such fevers to miasmatic exhalations. The latter writers know, fully as well as their opponents, that the existence of a marsh is not indispensable to the manifestation of the effect in question; but, unlike them, they are perfectly aware of the fact that morbific effluvia, of the most deadly character, too, may and do arise from sources which bear no resem-

[1] Des Fièvres Interm. et Cont. 1833, and Gaz. Méd. 1840.

[2] Ferguson on Marsh Poison, in vol. of Recollections, 185; Davy on Topography of Mediterranean, ii. 247, 248; Humboldt, Personal Narrative, iii.; Charles Darwin, Voyage of a Naturalist, ii. 129; Carpenter on Periodicity, New Orleans Journal, iii. 429; Caldwell, Essay on Malaria, 60, 61; Craigie, Practice of Physic, i. 87; Brown, Med. Essays, 33, 39; Currie on Bilious Fever, 55; Tulloch, Sickness, &c. of British Army on West Coast of Africa, 4, &c.

blance to a marsh. In the words of an intelligent writer, we may say: "Marshes and swamps are far from being the only sources of miasmata." "The foul shores of the sea; the moist slime and mud of the banks of great rivers, and of mill-ponds; the mire and mud in the unpaved streets, ditches, lanes, and passages of great towns and cities, villages, &c., particularly the cellars and damp abodes where the poorer classes are most frequently doomed to dwell—the moats of garrisons, &c.; the soil where certain hospitals, barracks, or encampments are situated; the wells and cellars, damp cells and dungeons of prisons, and the holds of ships, are all calculated to emit pyrexial effluvia from the moist earth, mud, and filth, which are mostly to be found within their precincts."[1]

The non-detection of malaria in the atmosphere no proof of its non-existence and agency.—It has been urged time after time, in opposition to the malarial origin of fever, that the existence of the effluvia, to the agency of which the disease is ascribed, has never been proved—that their presence in the atmosphere has been inferred from the effects observed, rather than positively demonstrated—that they have so far eluded detection; and that the air of sickly localities, whenever subjected to chemical analysis, has been found to contain the same ingredients, and in nearly the same proportions, as that of salubrious places. Much of this is doubtless true, and will not be denied by any one who has endeavoured to make himself acquainted with the state of knowledge on the subject. All are aware, for the fact has been often referred to, that the experiments of Gattoni, and others, led to the conclusion that the air of the marshes of Fuentes is as pure as that of Mount Legone,[2] one of the Grison Alps, at an elevation of 8,040 feet above the level of the sea. They are aware that, at Martinique, Moreau de Jonnes found no difference between the chemical composition of the air of Mount Tartanson, at a height of some six hundred feet above the level of the sea, and of the valley of Case Navire; and that the atmosphere of the wards of the hospital of Fort Royal, during the prevalence of yellow fever, differed from neither.[3] They know that Julia de Fontenelle could detect no deleterious gases or foreign chemical ingredients in the atmosphere of infected localities—that

[1] Blackmore on Infection, 92.

[2] Memoirs de la Société de Médecine, x. 109. Art. VI.

[3] Monographie de la Fièvre Jaune, 229.

the latter did not vary from that of healthy places in any of the principles which chemical analysis enables us to discover—a result he obtained in the marshes of Cercle near Narbonne, at the pond of Pudre, at Salces, Salanque, Capestang, Cette, at Barcelona during the fever of 1821, and at Paris during the cholera of 1833.[1] They also know that a distinguished chemist and professor in the school of Montpellier, Berard, reached the same conclusions from his experiments on the pestiferous air of the marshes on the coast of Cette;[2] and that Desaye obtained the same elements in the most confined marshes as on the most exposed hills.[3]

They are aware that, according to more recent authorities, the proportion of oxygen is the same in very high regions as it is near the surface of the earth.[4] They are, besides, conversant with the fact that, in more than one hundred analyses made in Paris and its environs, the least quantity of oxygen found was 20.913; the greatest, 20.999, and the mean, 20.96; that at Montpellier, Lyons, Berlin, Madrid, in Normandy, and Switzerland, the quantity varied from 20.903 to 21.000; that in the port of Toulon, in the middle of the Mediterranean, at Algiers, in the Atlantic, between Liverpool and Vera Cruz, the results were the same; that in the village of Gualla-bamba (Republic of Equador), the quantity was discovered to be 20.960, and on the summit of Pichincha, 20.949 : 20.988; that, in eleven specimens of air collected in the southern seas, only two gave results differing somewhat from the above—that of the Gulf of Bengal, where the quantity was 20.46 : 20.45; and of Ganges, where it amounted to 20.390 : 20.387; and that in the Polar Seas, according to Captain Ross, similar observations were made in respect to the composition of the atmosphere.[5] They perceive from these results, as well as from those obtained by Levy and Brunsen, that the variations in the composition of the atmosphere, wheresoever examined, are exceedingly limited, the difference in regard to the volume of oxygen being from 20.9 to 21; that the composition of the air is the same in the highest attainable strata of the latter as on the surface of the earth; and that if, in some instances, espe-

[1] Recherches Hist. Chim. et Méd. sur l'air Marécageux, 91, &c. See also his translation of Mojon's Essay on the Animalcular Origin of Cholera, 3, 4.

[2] Julia, *op. cit.* 93.

[3] Cyclop. of Pract. Med. iii. 60.

[4] Becquerel, Des Climats et de l'Influence qu'ils exercent sur les Sols boisés, &c. 2, 3, 4.

[5] Regnault, Comptes Rendus, &c., de l'Académie des Sciences, xxxiv. 867.

cially in hot climates, the quantity of oxygen lowers to 20.3, with a variation of about 0.020 of its volume, it may be doubtful whether such a small difference can have any influence on the phenomena of organic life or on health.

"The Almighty," says a well-informed writer, "has not permitted the chemist to discover the nature of such attenuated exhalations: they elude all detection; for if he take a volume of stagnant air from the foul 'plague ward' of an Egyptian hospital, where crowds of living and cadaverous beings are hourly stricken with the agonies of death, his analysis will prove it to contain the exact proportion by weight and by measure of elements and compounds, as those contained in an equal volume of a balmy breeze taken from a free and open English valley, where all are smiling with the inestimable blessing of health, and glowing with its bronzed and ruddy hues."[1]

Fever not due to the action of any known gases.—Aware of all these failures in the attempt to refer the cause of fever to any disproportion in the natural gaseous compounds of the atmosphere, we can find no difficulty in acknowledging the erroneousness of such opinions as that of Dr. Currie, who, basing his conclusion on sundry long-forgotten experiments of Vanbreden, thought that the insalubrity of low and moist places is not owing to invisible miasma or noxious effluvia, but to a deficiency of oxygen resulting from animal and vegetable decomposition;[2] or of the once famous doctrine of the Septon—an undiscovered compound of azote and oxygen, as propounded and defended by the late Professor Mitchell, of New York; or more recently, of the theories of our countryman, Rumph, of Giannini, and others too numerous to mention. Few, also, who have reflected seriously on the subject will feel disposed to lend a willing ear to the theory which teaches that the cause of fever must be sought in the admixture, in minute or larger proportion, of sundry gases, with or without addition of other substances—carbonated hydrogen, carbonic acid gas, ammoniacal gas, hydrosulphuric acid gas, phosphuretted hydrogen, &c.;[3] for, were it true, that these

[1] Griffith's Chemistry of the Four Seasons, 225, 226.

[2] Philadelphia Philosophical Transactions, iv. 135.

[3] Ramazzini; Volta Opera, 3 Florence, 1816; Dumas; Pallas, 219; Faust, Amer. Journ. vi. 38; Thénard and Dupuytren in Monfalcon, 54; Baumes, Emanations Marécageuses, in Monfalcon, 53, 54; Deslandes, Dict. de Med. prat. article Emanation;

gases have been, or may be, detected in the atmosphere of sickly localities, it would not follow that we must ascribe fever to their agency, seeing that though often unpleasant to the smell, and sometimes injurious or even fatal in their tendencies, they do not produce phenomena analogous to those of the diseases in question, even when absorbed in large quantities; while those they do occasion present always a widely different garb; and that, as they do not produce the symptoms of true pyrexial complaints when absorbed in large quantities, they are not likely to occasion them when received in such small proportion as to elude detection. If they were the legitimate cause of fever, and the active agents of miasma, it is impossible to understand how the fact could not have been demonstrated during wide-spreading and highly malignant epidemics, when the cause was acting with intense energy, and must have existed in sufficiently large proportion to be detected by some or other of the means within our reach. It should be borne in mind, too, that, when fatal, their effects are rapid, often instantaneous; that when, on the contrary, they do not occasion death, the immediate results very usually pass off, leaving the sufferer sometimes more or less debilitated, but without one febrile symptom; and that many of them are appreciable to the senses, and if existing in injurious proportions, would soon be detected. It is not less true, and to the purpose, that such gases may be produced or exist anywhere, in sickly as in healthy localities; while fevers, on the other hand, occur in certain places only, within certain latitudes, and under special circumstances; that, in many situations, where fever prevails extensively, the existence of some of those gases, to any notable or injurious amount, has not only not been demonstrated, but is impossible, or improbable; and *vice versa*, that, in places where they are abundantly produced, fever never shows itself. Even hydrosulphurous acid gas itself, on which so much has been said lately in France by M. Chevreul, in England by Daniel, and in this country by Dr. Gardiner, in its relation to the etiology of autumnal fevers, will not be found, on reflection, to afford much aid in accounting for those diseases. True, it may be, as Chevreul has shown by direct and positive experiments, that this gas is formed by

Balme, Traité de la Contagion, 305; Chevreul, Bulletin de l'Acad. de Méd. xviii. 692; Daniell, Lond. Med. Gaz. xxviii. 669, 700; Gardiner, Amer. Journ. N. S. v. 279; Melier, Mém. de l'Acad. de Méd. xiii. 492; Carrière, Le Climat de l'Italie, 311, 312; Humboldt, Personal Nar. iii. 188.

the action of the sulphurets contained in water or the earth on organic matter, whether animal or vegetable, with which they come in contact (by which these nearly insoluble and inoffensive substances are converted, through means of the combination of oxygen with the azotized matter, into soluble sulphurets), a result which has enabled M. Fontan[1] to explain the formation of sulphurous mineral waters. True it is, also, that those sulphates, together with organic matter, are found in most, if not in all sickly localities, and hence that the gas in question is also encountered there in a greater or less amount; but it is not less true that, in many places, subject to fevers, and during wide-spreading febrile epidemies, of various forms, from the simple intermittent to the malignant yellow, the presence of this supposed agent is not evident to the senses, or to chemical reagents, and must, if it exist at all, do so in so small a proportion as to be inert. It should be borne in mind, besides, that hydrosulphuric acid gas proves innocuous in factories and bathing establishments[2] where it abounds; and that its presence in the African rivers and circumambient air, on which so much stress was laid by Dr. Daniell and others,[3] has been positively disproved—water newly taken up, or kept in bottles, hermetically closed, never giving evident signs of that presence, which is evidently due, after awhile, to the putrefaction of the organic matter contained in the water.[4] In a word, there is not the most distant probability that malaria will ever be found to owe its morbific agency to, or consist in any extraneous gas floating in the atmosphere, or, that fevers are due to an excess or deficiency of one or more of the known constituents of the latter.

Not true that nothing is found in the atmosphere of sickly localities.—All this we must admit; but while doing so, we can find no valid reason for denying the very existence of a specific febrific cause; for from the circumstance that malaria cannot justly be identified with any of the gases above mentioned, it does not follow that the

[1] Bulletin de l'Acad. x. 692, &c.

[2] Annales d'Hygiène, xi. 290; Brocchi Stato Fisico di Roma, 254, &c.; J. K. Mitchell, 28.

[3] *Loc. cit.*

[4] M'Williams, Medical History of the Expedition to the Niger, &c. 172; Pritchett, Some Accounts of the African Remittent Fever, 117; Edin. Journ. 63, 442; Armstrong, Sulphuretted Hydr. and its Antidotes, Naut. Mag. 1842, 378, &c.

atmosphere of sickly localities contains no extraneous material to which autumnal fevers are to be ascribed. Certain it is, that those who enter into the investigations unbiased by preconceived notions will not lose sight of certain facts which will probably one day be found entitled to the particular attention of the medical inquirer, and lead ultimately to useful conclusions on this important matter—not, perhaps, by giving us at once correct notions respecting the real nature of the cause, or enabling us to seize the poisonous matter and subject it to chemical analysis, but by determining its general nature and the class of substances to which it belongs. The medical reader will recollect that Moscati, the first who suggested the idea of condensing the water dissolved in the atmosphere of insalubrious places with a view to discover the effluvial principle, obtained by that process, in sundry experiments he instituted at Milan, on the air of rice grounds, and of the wards of the large hospital of that city, deposits of a flocculent matter, emitting a cadaverous odour.[1] Brocchi, at Rome, found albuminous flakes (animal matter) in the dews of the Pontine marshes.[2] In 1812, Rigaud de Lisle, experimenting on the marshes of Languedoc and Provence, collected a quantity of the dews which, when examined some five or six months after by Vauquelin, was found to contain flakes of animal matter; while the experiment of the former on *fresh* dews, collected from marshy surfaces, gave a somewhat different result, exhibiting, as they did, common air without admixture of any gas, but containing alkaline salts, with vegetable and animal substances.[3] Dumas, and before him Volta, found an organic substance combined with the gases disengaged from stagnant water. Julia de Fontenelle and Herpin, obtained results differing but little from those recorded by Brocchi, so far as regards the flocculent or inorganic matter, while in common dew nothing of the kind was discovered.[4] Agreeably to the first of these experimenters, the air of marshes may be inodorous, but if kept six months, it acquires a nauseous smell—an effect not noticed in common air.[5] Ozanam also found "a substance apparently mucous, which emitted

[1] Compendio di Cognoz. Veterin, 81; Monfalcon, 59; Levy, Hygiène, 2d ed. i. 444.

[2] Dello Stato Fisico del Suolo di Roma, 259–275.

[3] Annales Cliniques de la Soc. Prat. de Montpellier, xliv. 286; Julia, *loc. cit.* 83–84. An account of these experiments was addressed to Pictet, and published in the Bibliothèque Universelle.

[4] Julia, 86–7.

[5] *Ibid.* 90.

a very fetid odour."[1] Boussingault, whose experiments were made in the department of Ain (France), in 1819, and subsequently in South America, on the banks of the Taricagua, and at Cartago, in the valley of the River Cauca,[2] demonstrated also the presence of organic matter in the air deposited with the dew. This matter imparted a dark hue to concentrated sulphuric acid exposed to miasmata during the night—the quantity varying according to the unhealthiness of the seasons at which the experiments were made. Thenard and Dupuytren found that the carburetted hydrogen obtained from marshy grounds, when passed through water, deposited therein a peculiar putrescible matter—a result not obtained from the same gas disengaged in the ordinary way.[3] In a communication made to the French Academy of Sciences, in 1847, respecting sundry observations and experiments made by him on the condensation of fogs and dews, M. Gasparn states that from these a peculiar matter was obtained, which, on trial, was found detrimental to health, and fatal to sheep. The same results were obtained by M. Malagutti, an Italian chemist.[4]

In 1828, Messrs. Meirieu condensed the dews collected over certain marshes situate in the department of the Gard, and obtained therefrom a peculiar substance possessing acid properties.[5]

Liebig tells us that "all the observations made upon gaseous contagious matters, prove that they also are in a state of decomposition. When vessels filled with ice are placed in air impregnated with gaseous contagious matter, their outer surfaces become covered with water containing a certain quantity of this matter in solution. This water soon becomes turbid, and, in common language, putrefies; or, to describe the change more correctly, the state of decomposition of the dissolved contagious matter is completed in the water."[6] Finally, a physician of our own country, Dr. Hume, Professor of Elemental Philosophy in the State Military Academy of

[1] Hist. Méd. des Maladies Epid. i.

[2] Recherches sur la composition de l'atmosphere, sur la possibilité de constater l'existence des Miasmes, Ann. de Chimie et de Phys. lvii. 148, &c.; Gaz. Méd. Aug. 16, 1834; Am. J. xv. 544; do. xix. 263; Archives, 2d S. v. 641.

[3] Monfalcon, 54.

[4] Becquerel, Traité d'Hygiène, 174, 183; Gaz. Méd. de Paris, ii. 3d series, 1847, p. 22; Anglada, Traité de la Contagion, i. 34–5.

[5] Influence des Miasmes Marécageux, sur l'Economie Animale, Montpellier, 1829, p. 9, referred to by Anglada, *op. cit.* p. 34.

[6] Agricultural Chemistry, Lond. ed. 373; Am. ed. 407.

Charleston, during some experiments made by him in that city a few years ago, discovered the existence of an organic matter suspended in the atmosphere of localities infected with the poison of the yellow fever. "Whether animal or vegetable," says Dr. H., "it is impossible to determine; but I am inclined to suspect the presence of both, as the odour was more like that of animal matter, while the charring and subsequent combustion of the carbon was indicative of vegetables. The positive detection of organic matter in the condensed water of a presumed infected cellar, in a decidedly infected district, is a new fact in the etiology of the disease, and points distinctly to the origin of our yellow fever."[1]

Whether, in the present state of knowledge on the subject, we can unhesitatingly connect the production of malarial fevers of all grades and types, with the existence of azotized flakes in the atmosphere—attributing, with Julia (pp. 121, 153), Fourcroy,[2] and others, the morbid effects noticed in sickly localities to particles of animal and vegetable matter in a state of putrefaction, and mixed with, and floating in, the air—I will not pretend to decide in a very positive manner. Perhaps the fact adduced on some occasions, that analogous discoveries have been made in air issuing from noted sources of vegetable and animal putrefactions, and which, nevertheless, did not give rise to malarial fevers, and that, on the contrary, chemists have not unfrequently failed to detect azotized flakes in the air of localities where fever prevails more or less extensively, may deter us from regarding them as exclusive agents in the production of those diseases. But be this as it may, the results obtained in a large majority of the trials made in various situations, prove very clearly that in marshy places, during the precipitation of dew, flakes of organic matter are deposited with it; that, in many instances, a similar matter has been obtained in the atmosphere of infected places; and surely the frequency of the occurrence, taken in connection with other circumstances that will be noticed presently, justifies the conclusion that the researches of chemists have not been as barren of results as is usually affirmed by the opponents of malaria, and that the azotized matter obtained during the prevalence of fever may very reasonably be supposed to have some agency in the production of the latter.

[1] Charleston Med. J. v. 24–6.

[2] Putrefaction des Substances Animales, quoted by Julia, 122.

Chemists not more successful in discovering other morbid poisons in the atmosphere.—But let us admit that chemists have failed, in all places, and under all circumstances, to detect in the atmosphere of insalubrious localities something tangible to which the causation of fever may justly be ascribed; let us also admit, for the sake of argument, that there exists no connection, as cause and effect, between the azotized and putrid flakes above referred to, and the diseases in question, and that we are as far off now as our forefathers were centuries ago, from the possibility of demonstrating, in a satisfactory manner, the nature of the malarial poison, and to prove its independent existence; it is doubtful, as every attentive reader will perceive, whether the opponents of the malarial origin of autumnal fevers can derive from that acknowledgment an overwhelming and convincing argument in favour of their views. He must at once see, that if we deny the existence of febrile miasmata on the ground that no one has as yet satisfactorily succeeded in detecting them in the atmosphere, we shall be called upon to make, for the same reason, and for the sake of consistency, other denials, for which, I presume, few physicians, whatever be their opinions on the subject before us, can be prepared. Let us not forget that no one has as yet been able to detect the poison of other zymotic diseases—small-pox, scarlatina, measles, influenza, hooping-cough, typhus, Asiatic cholera, plague, &c., contagious or otherwise—and yet we know that they must at times float in the air, since they produce their respective morbid effects in individuals who breathe that tainted medium; and under circumstances, too, which forbid the supposition that those attacked could have received the infection by direct exposure to, or contact with, the sick. Liebig, with all the facilities afforded by the appliances of the Giessen laboratory, and all the uncommon dexterity and analyzing powers he is known to possess, tried the experiment, but failed. The aroma of flowers, and other odorous substances, betray their presence in the atmosphere, through their effects on the olfactory nerves; and yet, Cavendish and other able operators found no difference between pure air and that in contact with such substances; and were it not for hc effects in question, and sometimes for the impression they produce on the nervous system at large, they would remain undiscovered. We are told by Lind, that turners, in working the wood of the manchineel tree, would be severely affected, did they not securely guard against breathing its poison. A field of poppies has been known to induce a sleepy

disposition in the bystanders. Van Swieten, in the early part of his life, suffered repeatedly a temporary loss of memory from the vicinity of a plant to him.[1] The air collected in the wards of the hospitals of Milan, Paris, Barcelona, and other places, in dissecting-rooms, in knackeries, in cemeteries, in the galleries of a crowded theatre, from cesspools, culverts, stables, or collections of putrid meat, must naturally be supposed to be impure; nevertheless, as seen, it does not differ from the common atmosphere in any material point. In some of these instances, a little ammonia or carbonic acid may be found; but the result is not constant, and is, besides, of no importance,[2] for the deleterious properties of infection continue unimpaired after these substances have been removed. Nor can chemists discover any difference between the pure air of the loftiest mountain ranges, and the air through which a dog can tell that a horse, a fox, or a man has passed.[3] Experiments have made it clear that some morbific vapours may be diffused in the atmosphere in such attenuated proportions as not to be detected by the smell or by chemical analysis, and yet occasion baneful and even deadly effects, not only on some of the lower order of animals, but also on the more elevated in the zoological scale.

In addition, it may be remarked that medicines, even purgatives, are sometimes found to act through the medium of the air, in which they cannot, on analysis, be detected. In all these instances, the substances in question, though defying the scrutinizing efforts of the experimental chemist, are well known to exist in the atmosphere; some by their peculiar odour, others by having been placed there purposely, and most of them by their effects on the economy; and, surely, if such is the case in regard to those substances, there can be no reason to refuse admitting others possessing toxicological properties the same privilege of concealment, and to deny their presence on the score of their not being detected on chemical analysis, seeing, as we do, that effects, palpable enough in all conscience, follow closely on exposure to an atmosphere which many circumstances lead us to regard as being contaminated. From analogy, therefore, we may conclude, that though escaping, in consequence of their inodorousness, and the inefficiency of our means of analysis, the notice of casual observers, and no less the researches

[1] Means of Preserving the Health of Seamen, 24–5, note.

[2] Guyton Morveau, Ann. de Chimie, xxxix. 84.

[3] Barton, Report to the State Medical Society of the State of Louisiana, 26.

of the chemist; malarial effluvia may, nevertheless, float in the atmosphere, in sufficient amount and in a sufficient state of concentration to manifest their presence by occasioning their specific morbid effects—febrile diseases—in individuals exposed to the influence of the atmosphere which they serve to contaminate.

To those who adduce such an objection, it may be remarked that no one will venture to doubt or deny the existence, in the matter obtained from a variolous or vaccine pustule, or in the pus of a chancre, or in any other animal poison, of a particular virus capable of infecting the system, and reproducing in it a specific disease similar to that whence that virus proceeds. The variolous matter produces smallpox, the vaccine matter occasions cowpox, the pus of chancre gives rise to syphilis, &c. Well, let them inquire how far chemistry has gone in the way of discovering the nature of these particular poisons, and the particular element through which they derive their toxicological powers, and they will find little calculated to satisfy their curiosity. On this subject the science, so far, has not effected much more than it has in regard to the atmosphere of malarial localities. The analysis of the pustules of mild and simple smallpox, gives only fibrine, mucus, hydrochlorate of soda, sulphate of potassa, phosphate of lime, and water. That of pus furnished by smallpox complicated with petechial eruptions, gives the following materials: Fibrine, mucus, hydrochlorate of soda, hydrocyanuret of soda, sulphate of potassa, phosphate of lime, and water.[1]

Not one of these materials can by itself produce smallpox, however introduced into the system. Combine them together artificially, in whatever proportion you please, and you will not be likely to occasion the effect. And yet they are the only materials found by the chemist; while the particular *something* which imparts an individuality to the poison, and constitutes it a specific agent, has fled. Will any one deny, on that account, its presence during the analysis? The same may be said in relation to vaccine, the predominating elements of which consist in water and albumen; though, on more minute examination, it is found to contain a matter similar to osmazome, chlorate of sodium, chlorate of potassa, and phosphate of lime. Here, again, the peculiar something has vanished.[2] Similar

[1] Journal de Chimie Médicale, 1828, iv. 488; Anglada, Tr. de la Contagion, i. 205.
[2] L'Heritier, Traité de Chimie Pathologique, 612; Anglada, i. 204.

remarks are applicable to the virus of chancre, of gonorrhœa, of hydrophobia, of glanders, as also to the extensive class of vegetable poisons, all of which, when chemically analyzed, furnish, in various combinations and in various proportions—the first the ordinary constituent elements of animal, the other those of vegetable substances—but in no instance present us with the peculiar ingredient which imparts to each of those products its specific poisonous property.

Again, it has been said that the blood of individuals affected with measles communicates the disease, when inserted into incisions made in the skin, and kept there by means of cotton. I say, it has been said; for though the experiments which Dr. Francis Home, of Edinburgh, instituted on the subject, about the middle of the last century, and those of Speranza, during the epidemic of Milan, in 1822, would seem to justify the conclusion, and though the high authority of Stoll may be adduced in its favour,[1] the success of this inoculation is yet open to doubt. But, admitting it to be true, and admitting, also, what can scarcely be denied, that in measles, as well as in the whole tribe of zymotic diseases, the blood is the vehicle of the poison, in some form or other, it is a well-ascertained fact that, in the present state of the science, chemistry is powerless in its endeavours to discover in that fluid any anomalous element, in the least indicative of the presence there of the toxicological agent to which the disease is due.[2] Examine also the blood of individuals labouring under scarlatina, smallpox, vaccine, typhus, typhoid, cholera, or other disorders in which a peculiar poison has been at work, and the fluid has been morbidly affected, and you will be sure to fail to discover in it traces of the presence of that poison, of whose existence, nevertheless, there can be no doubt. In such blood the quantity of water, fibrine, corpuscles or residue of the serum may vary more or less from that contained in the healthy fluid. The solidity of the clot may differ,[3] but in no instance can we detect the specific *materies morbi.*

It is not a little surprising, if not amusing, to find that while, by ignoring the presence in the atmosphere of a particular matter which, by its poisonous property, is supposed to give rise to fever,

[1] Aphorism 583, p. 202.

[2] See the experiments of Andral and Gavarret, and of Becquerel and Rodier; see also Bouchat, Maladies Contagieuses, Gaz. Méd. 1848; Simon, i. 300.

[3] Simon, i. 288, 298, 325.

and to which the name of malaria has been applied; in other words, while, by denying a separate existence to febrile effluvia, on the ground that chemistry has not succeeded in detecting them, the disbelievers in that poison seem anxious to vindicate the character of the science for infallibility, or at least to uphold the idea that it has reached, on the subject in question, its culminating point, and that its not having succeeded in attaining the object in view, indicates positively the impossibility of this ever being effected, professional chemists of the highest eminence, so far from carrying their pretensions that high, acknowledge the deficiency of their present means, admit that their science is yet in its infancy, and do not hesitate to say that the want of success in the detection of malarial emanations is not to be taken as an evidence of the non-existence of these. The celebrated author of the *Statique Chimique*, Bertholet, somewhere says: "Besides its constituent parts, the atmospheric air may contain in solution different substances, which assume in it the elastic form, and of which some are principles of odours; but so far these emanations have eluded chemical means, which can destroy some, but not detect them."[1] Similar admissions are made by Raspail,[2] Muldor, Van Gaens,[3] and others; but by none more clearly than Sir H. Davy, who, while expressing the conviction that, by the progress of chemistry and physiology, success will ultimately be obtained on this point, says: "That a specific matter of contagion has not been detected by chemical means in the atmosphere of marshes, does not prove its non-existence. We know so little of those agents that affect the human constitution, that it is of no use to reason on the subject."[4]

Nature and condition of fever localities lead to the opinion of the existence of malaria.—The opinion which ascribes autumnal fevers to the contamination of the atmosphere by poisonous effluvia, receives not a little support from their very frequent association with peculiar characters and conditions of the localities where they are noticed, and from their absence or cessation where those characters and conditions are wanting, or have been removed by artificial or other means. Whenever we encounter such localities—characterized by a geological formation of tertiary and cretaceous secondary deposit

[1] Vol. i. [2] Hist. Nat. de la Santé et de la Maladie, i. 40.

[3] Quoted by Harrison, New Orleans Journal, ii. 582.

[4] Consolation in Travel, Works, ix. 582.

with argillaceous and rich alluvial soil more or less impervious to water; or where, whatever be the appearance of the soil, water is found at a very short distance from the surface; or where the latter is dotted with marshy fields, and traversed by sluggish streams; or by swampy, low, flat level land, as along the banks of lakes and winding streams; or by level plains, ravines, or deep valleys, either dried or drying on the surface, after having been thoroughly wet, and perchance encased between mountain elevations, covered, as in India, with jungles; or, in the West Indies, with impenetrable mangroves or thick forests, and containing a large amount of organic remains—there we may be very certain to meet with fever. "It is a safe generalization to affirm," says Dr. Drake (p. 709), "that, all other circumstances being equal, autumnal fever prevails most where the amount of organic matter is greatest, and least where it is least." In localities thus characterized, I repeat, we may almost certainly count on meeting with malarial fever in some one or other of its varied forms, after a certain continuance of high atmospheric heat. In others, differently circumstanced, we may safely anticipate an exemption from that disease.[1] The salt marshes of Normandy—about Doll, for example; the soil along the Mediterranean coast of France; the shores of the Adriatic, of Greece, and of Sicily; those of Sardinia, of Spain, of Crimea; the lagunes of Holland, from the Walcheren to Groningen; the soil of Flanders; the Pontine marshes of the Pontifical States, and the campagna in the vicinity of Rome; the maremme of Tuscany, Lucca, and the Mantuan; the soil of the Bresse, of Sologne, of Dombes, of the vicinity of Camargue, Aigues Mortes, Marenne, Brouage, Rochefort, and other parts of France; the coasts of Batavia and Madagascar; the alluvial districts of the western and eastern coasts of Africa; those of Algiers, and of India; a large portion of the United States, from the Delaware to the Mississippi on the Atlantic coast; along the margins of the lakes, the Mississippi River, and other western streams; a large surface in South America and in the West

[1] What is termed peat bog, or peat moss, is said not to be productive of malaria. Many parts of Scotland and Ireland, that are occupied by large tracts of marsh, in which peat moss abounds, are completely free from these fevers. The disease is never seen among the inhabitants near the country of the Dismal Swamp, on the frontier of Virginia and North Carolina. In them, the vegetable matter is subcarbonized, and hence insusceptible of decomposition. It is known to possess peculiar antiseptic qualities, which not only preserve trees and other vegetable, but animal substances, from putrefaction. See Acton's Treatise on Moss Earth.

India Islands; along the sea-coast, or the borders of tropical streams or lakes; the river banks and low flat surfaces of Hungary; the morasses of Upland, the plains of Scanea, Sudermania, and Gothia; all these localities, and many more that might, if necessary, be mentioned, and which are or should be familiar to every one who has made paludal or autumnal fever the subject of inquiry, are in point. They all present some one or more of the characteristics pointed out, and are all, at particular seasons of the year, and under certain atmospheric conditions, the seat of febrile affections; while other places, sometimes at no great distance, but of a different geological formation—less swampy or less rich in organic materials, and possessing a different soil—are, though similarly circumstanced in other particulars, free from the disease. All these facts serve to illustrate the almost constant connection existing, as cause and effect, between the kind of locality to which I have referred and the disease in question.

Nor is it less certain that the yellow fever proper is traced almost invariably to city districts noted for filth and imperfect ventilation; to the vicinity of ships, docks, or wharves; to narrow and confined courts and alleys not far from these; to collections of substances, animal and vegetable, in a state of decomposition, &c. Instances to that effect have been too frequently observed, and are too well authenticated to be denied; while the connection between the existence of places of the particular kind mentioned, and the appearance of that form of fever, is too constantly found to prevail, to be considered simply in the light of a coincidence.

The danger of an attack of fever increased in proportion to proximity to such localities.—If we approach to, or remain some length of time—occasionally only a few hours or moments—in those localities, or in their immediate vicinity, we are stricken down with fever; if we avoid them, we escape. The South Carolinian gives up his plantation residence in summer, and removes to Charleston or to the mountains, where he is safe from the country fever. Let him visit his estate before the advent of frost, and especially let him sleep there, and he runs great risk of an attack. In yellow fever seasons, strangers must leave or abstain from entering the city; if they venture into it, they will in all probability have the disease. With us, in Philadelphia—as with the residents of other cities of the Middle and neighbouring States, and of some parts of

Europe—where infected districts are of limited extent, the disease is restricted to individuals who venture within the bounds of these. At a very few paces from the sickly spot to which they penetrated, and where they doubtless imbibed the seeds of the fever, people move about, business is transacted with perfect impunity, and everything often looks precisely as if the city were not the seat of an epidemic. By avoiding our river banks, or our meadow or marshy land, by remaining within the limits of the city, or selecting high and dry situations in the country, Philadelphians, like Charlestonians, keep free from chills and fever, or remittents; by adopting a different course, they expose themselves to an attack. On the coast of the West Indies, and of Africa, as well, indeed, as in this country and Europe, vessels remain healthy so long as they keep at a distance from land.[1] But woe to them if, during the sickly season, they approach the shore, or enter the river streams. The moment they do that they become liable to the disease. Lind, in his work on preserving the health of seamen, states that when Commodore Long's squadron, in the months of July and August, 1744, lay off the mouth of the Tiber, it was observed that one or two of the ships which lay nearest the shore began to be affected by the pernicious vapour from the land; whilst some others, lying farther out at sea, at but a very small distance from the former, had not a man sick at the same time. (P. 67.) While in the autumn of 1852, many British steamers and vessels of war had the yellow fever for going into port at St. Thomas. "Another of her majesty's ships, the Devastation, was at St. Thomas, but did not come into the harbour, keeping, however, only about a mile off the town, and remained intact.[2]

"I have known," Sir G. Blane remarks, "a hundred yards in a road make a difference in the health of a ship at anchor, by her being under the lea of marshes in one situation and not in the other."[3]

[1] Lind. on Hot Climates, 138–178–9; *Ibid.* on Seamen, 78; Trotter, i. 456; Rouppe, 65—English translation, 69; Rush, iii. 83; Bancroft, 171; *Ib.* Sequel, 166; Clark on Long Voyages, i. 124; Moseley, 57; H. McLean, 26; Ferguson's Recollections, 151; Gillespie, 20; Fontana, 12; Bally, 455; Pringle, 57, 98; Chervin's Letter to Dr. Monfalcon, 12; Burnett, 264–274; Caldwell's Prize Diss. 139; Caldwell's Essay on Mal. in Boston Journ. 510; Williams's Morbid Poisons, ii. 446; Smith, Edinburgh Journ. xxxv. 49; Amiel, Edinburgh Journ. xxxv. 264; Johnson, Charleston Journ. iv. 160.

[2] Wiblen and Harvey on Yellow Fever, Lancet, April, 1853, Am. ed. 322.

[3] Diseases of Seamen, 228.

It is stated by masters of ships that during the prevalence of the late fever epidemic in Brazil, though they came direct from Europe, and held communication with no vessel of any kind on their passage, the disease made its appearance on board their ships as soon as they approached the coast, and came within the influence of the land breeze. Dr. Gavin states that when the yellow fever broke out in Georgetown, Demarara, at the end of 1851, some seamen arriving from Europe were attacked with the disease on nearing the coast and getting into the muddy water, some days before their arrival in harbour.[1]

Lind states, in his well-known work on Hot Climates, that "many persons escaped the yellow fever which prevailed in Pensacola, in 1765, by retiring to the ships which lay in the harbour." (179.) In another place the author remarks: "When the violent and fatal sickness raged at Cadiz, in 1764, it did not extend its influence to any ship which lay at a distance from the city." His majesty's ship, the Tweed, which was then at anchor in Cadiz Bay, like others, escaped. All the sick that were sent on board recovered, no bad symptoms appearing in their fever, "while a disease similar to the black vomit and the yellow fever, and equally mortal, depopulated that large city." (*Ib.* 178.) Commodore Mitchell's fleet, which anchored in the year 1747, between South Beveland and Walcheren, were perfectly healthy, while the soldiers, on shore, at the same time, and at no great distance, were sorely afflicted with fever."[2]

Dr. Rush says, of the epidemic of 1793: "I heard of some seafaring people who lived on board their vessels, who escaped the disease."[3] Dr. Caldwell remarks "that marsh malaria cannot reach the crew of a ship lying at anchor but a cable length from the shore, where it is generated:" and adds: "Similar facts may be collected from the history of yellow fever in our own country. During the prevalence of that disease in Philadelphia, many individuals, and several whole families, are known to have retreated to vessels lying not more than from two hundred to two hundred and fifty yards from the wharves, and to have remained healthy. In New York and Baltimore like instances have occurred."[4]

Who is unacquainted with the fact, noticed in diverse latitudes,

[1] Second Report on Quarantine, pp. 14, 15; London, 1852.

[2] Pringle, Rouppe, and Lind.

[3] *Op. cit.*

[4] Prize Dissertation, Boston Journ. iii. p. 510.

that while, in vessels at a short distance from infected localities, those who remain on board are exempted from fever, the boat crews, who, from the nature of their duties, are obliged to explore the river banks, those who land on business or for recreation, are, especially if they sleep on shore, sooner or later attacked.[1]

On the other hand, vessels that are infected in such localities lose the fever (if they themselves do not contain sources of infection) by shifting their position, and anchoring at a distance—sometimes at a very short one—from the shore; or by going to sea, and thereby placing themselves beyond the influence of the land air. The morbific agency (in Georgetown, Demerara) seemed to move in shifting swarms or vortices, hovering over a vessel here and there. "Thus, in the beginning of 1839, the Thomas King lay in the division A, between Kingston and Wighart's Stellings. In one week she lost four hands. She unmoored, and took her station *outside*, or to the leeward of the Louisa Baillie; the mortality ceased, and the health of the crew became re-established. The Louisa Baillie, that had been right abreast, and sheltered by the Thomas King before the unmooring, and had no death, although several cases of fever, which readily yielded to treatment, became soon very sickly; she lost four men, after which she shifted her moorings, and the mortality then ceased in her also.[2] These effects have been observed on frequent occasions in Europe, in the West Indies, on the coast of Africa, as well as on our western lakes.[3]

I am informed by Mr. Martin, surgeon of the Cananeuch, a Guinea trader (Dr. Lind states) that when he was in Gambia River, in company with four other ships, the men in one of those ships were daily taken ill of fevers and fluxes, and several of them delirious; while all the English in the other ships and in the factories were in

[1] Badenock, Med. Obs. and Inq. iv. 157–8; Trotter, Med. Naut. ii. 86; Bally, Typhus d'Am. 455; Clark on Long Voy. i. 38–40; Boyle, 75; Rouppe, pp. 65, 75; Gillespie, 20; J. Hunter, 17; Valentin, 77; Caillot, 200; Bancroft, 172; *Ibid.* Sequel, 166; J. Wilson, 66; Fontana, 12; Blane on Seamen, 92, 392; Lind. 106, 108, 134, 162, 195; Lind. on Seamen, 73, 77–8; J. Johnson, 63–5, 127, 134; J. Wilson, Stat. of Brit. W. I. Squadron, 85; Bryson, 151, &c. &c.; Burnett, 188–225, 268; Smith, Edinburgh Journ. xxxv. 13, 47, 50; Allon, Edinburgh Monthly Journ. Aug. 1847; Bryson, Stat. Rep. on the Health of the (Brit.) Navy, 215, 220, 230.

[2] Blair, 36.

[3] Lind. 200; *Ibid.* on Seamen, 85; Ferguson's Recol. 143; Trotter, i. 358; Hunter, 16; Keraudren, 18; H. McLean, 26; Rufz. Report, by Chervin, 60; Usher Parsons's Dissertations, 203; Cooke, Med. Recorder, vii. 451.

perfect health; but upon removing that ship about half a league from her first anchorage, which was too near some swamps, her men became as healthy as those in the other ships.[1]

Sir G. Blane remarks: "When ships watered at Rockfort (Jamaica), they found that if they anchored close to the shore, so as to smell the land air, the health of the men was affected; but upon removing two cables' length no inconvenience was perceived."[2]

These effects are not due to heat alone.—Now, how is all this to be accounted for? Notwithstanding all that has been said to the contrary by Towne,[3] Poissonniere,[4] Madrid,[5] Hosack,[6] Tommasini,[7] Faure,[8] Vatable,[9] Girardin,[10] Bertaud,[11] Bourdon,[12] Dickinson,[13] and many others, we may safely affirm that excessive, great, or long-continued heat, will certainly not do so. Unaided by a more efficient morbific agent it has in no instance been found to occasion a remittent, an intermittent, a yellow or a pestilential fever. It is a well-ascertained fact that in many localities where the heat is intense, fever is seldom if at all observed, while it prevails more or less extensively in places where the temperature is considerably lower. The summer heat of the southern portion of the great desert is very great; but, as Dr. Drake remarks, those who traverse it, and keep at a distance from its watercourses, pass the season unaffected. The same writer calls attention to the fact that the sandy banks of Pensacola Bay, from its entrance up to the town of Pensacola, suffer but little; while at the head of the bay, where extensive alluvial deposits have been made, the fever has been so constant and fatal as to prevent permanent settlement. Yet the temperature of both localities is the same, for they are but ten miles apart. In some localities, as the Antilles generally, fevers are said

[1] Lind on Hot Climates, 180.

[2] Diseases of Seamen, 178.

[3] A Treatise on the Diseases most frequent in the West Indies: and herein more particularly of those which occurred in Barbadoes. Lond. 1726, 11.

[4] Traité des Fièvres de St. Domingue. Paris, 1780, 50.

[5] Ensayo Analitico Sobre la Naturaliza, Causas y Curacion de las Calenturas, &c. Habana, 1821, pt. ii. 1.

[6] Practice of Medicine, 390.

[7] Sulla Febbre di Livorno, 1804; Sulla Febbre Gialla Americana. § 84. i. 168.

[8] Fièvres Intermittentes et Continues, &c. 43, 44.

[9] Ann. Maritimes, 1828, 330.

[10] Essai sur la F. J. 44.

[11] Dissertation sur les Maladies que les Médecins de St. Domingue ont appelée Fièvre Jaune, 7.

[12] Considérations sur la Prophylactique de la Fièvre Jaune. 7.

[13] Observations on the Inflam. Endemic, &c. or Yellow Fever, 84, &c.

to be now more rife than they were formerly, though the temperature was not lower then than it is at the present day. The savannahs or natural prairies of French Guiana, the arid deserts of Peru, and a large portion of Spanish Guiana, furnish us with examples of the sort.[1] Thermometrical observations made in the plains of Meta, situate on the east side of the oriental Cordilleras and in the valley of the Magnaline on the west of the same ridge, exhibit great similarity of temperature, yet the former is remarkable for insalubrity, while the latter is free from fever.[2]

The range of the thermometer in Antigua and Barbadoes, is rather higher than in Dominica, Tobago, Jamaica, or the Bahamas; yet we find that the troops in the latter stations suffer nearly three times as much as those in the former.[3] The yellow fever does not prevail on Pigeon Island, but is of common occurrence in the town of Castries,[4] though the temperature is much the same in both places. While common in some parts of the coast of Africa, the same disease is rare in Senegal.[5] It is rare also in Cayenne, which is, to say the least, as hot as any place within the tropics.[6] Panuco, and the plains of Coro and Cumana, and the coast of Coromandel and of Onixa, cited by Humboldt,[7] as well as the coast of Malabar, the deserts of Arabia, and of the Diabakie[8] are rarely the seat of malarial fevers, especially of those of a malignant character. The same may be said of Lima, Brazil, Peru,[9] the savannas or natural prairies of French and a large portion of Spanish Guiana,[10] where, in spite of excessive and long-continued heat, this fever does not exist.

The prevalence of and mortality from fever have also been found in the same place far greater in some seasons than in others—the temperature remaining nearly the same;[11] and it has originated and prevailed in some situations—as well in the West Indies, in Africa, in Europe, as in this country—nearly as often when the range of the thermometer was at the minimum of tropical heat as at the maximum.[12]

[1] Leblond, 6, 62–4–5.
[2] Boussingault, Annales de Chimie, lvii. 153.
[3] Report on Mortality in British Army, 101.
[4] Pugnet, 342.
[5] Thevenot, 244.
[6] Bajon Mém. sur Cayenne, 29, 59.
[7] Volume i. 62.
[8] Bonneau et Sulpici, Recherches sur la Cont. de la F. J. 16.
[9] Vicaire, Annales Maritimes, Oct. 1831, 298–9.
[10] Leblond, 6, 62–4–5.
[11] Sickness, &c., in British Army, 5, 101; Craigie's Practice, i. 224–6.
[12] Report of Sickness of British Army, 26; Cartwright's Recorder, ix. 226; Blair, 52.

This want of necessary connection, as cause and effect, between high atmospherical heat and fever—common autumnal and yellow—has been pointed out by a large number of writers on these diseases as they show themselves in various parts of the globe.[1]

Again, heat is greater in cities than in the surrounding country, and yet malarial fevers are more frequently encountered in the latter than in the former. And if the cause of yellow fever is more frequently evolved in cities, it is because it requires a higher degree of temperature. But even this will not serve to disprove my position; for, during the existence of the disease, many localities in the close vicinity of, and just as hot as, the part infected, remain healthy. Indeed, in insalubrious regions fevers are more particularly rife, not during the hottest months of the season, but some time after, when the average temperature has lowered in a notable degree. Such has been found to be the case in Gibraltar,[2] in France,[3] in this city,[4] in Charleston,[5] Barcelona,[6] the West Indies,[7] Algeria,[8] &c. It may be mentioned, also, that fevers often cease long before the cessation of hot weather—when, indeed, the temperature has reached its highest point. We know that such is the case in relation to the Egyptian plague, and that it has occurred also at Dantzic, Toulon, and Stockholm.[9] "There broke out a plague in Venice," says Matth. Villani, an eye-witness (*Lib.* i. *Historiar.*), "in the year 1348, in the month of March. It was at its height in April and May. It began to decline in July, and ended in August; so that a plague will end in hot weather."[10] The occurrence has been noted in reference to the yellow fever; and in South America, this coun-

[1] Copland, iii. 151; Bryson, 197; Dazille, Malad. des Nègres, 8, &c.; *Ibid.* des Pays Chauds, 4; J. Wilson, 66–7; Boyle, Ed. J. viii. 173; Warren, 8; Dariste, 32; Osgood, 18; Good, ii. 168; Chisholm, ii. 264; Dalmas, 38; Bally, 327; Gillkrest, 279; Musgrave, Med. Ch. Tr. ix. 121; Imray, Ed. J. liii. 92; Chalmers, i. 22; Lining, ii. 407; Townsend, 377; Hosack, ii. 29; Chervin, translation of Wilson on Fever of Gibraltar, 9; Sir J. Fellowes, 417; Hunter, 13; Ferguson, Med. Ch. Tr. viii. 142; Evans, 45; Davy, Notes to Blair, 52; Drake, 712–3.

[2] Report of Sickness in Mediterranean Station, 65.

[3] Nepple, Fièvre Interm. 135.

[4] Emlen, N. A. Journ. v. 328; Carey, 7.

[5] Lining, *op. cit.* ii. 410.

[6] Rochoux, 110; Pariset, 475.

[7] Williamson, Med. and Miscel. Obs. on West Indies, i. 210, 211; Pinckard, Notes on the West Indies, ii. 485; R. Jackson, Sketches, 9, 10, 11.

[8] Jacquot, Des F. à Quinquina, 19; Haspel, Maladies de l'Algérie, i. 67.

[9] A Treatise on the Plague and Pestilential Fever, 10, 11. Lond. 1751.

[10] *Ibid.*

try, Europe, and elsewhere, the same observation has been made, so far as regards ordinary fevers.[1] To this it may be added that, in localities where fevers have been driven away through the operation of judicious hygienic measures, the effect was not attended with a diminution of atmospheric temperature; that some forms of autumnal periodic fevers are restricted to very circumscribed localities, where the heat is no greater than in the immediate vicinity; and that when it breaks out in a ship and spares others close by, or when it ceases in a vessel on the latter's shifting its position, we cannot presume that a difference of temperature can have had anything to do with the difference of the results observed.

The following statements, derived from a report recently published by Mr. Lorin Blodget, who has charge of the meteorological department of the Smithsonian Institution at Washington, may be appropriately placed here.[2]

Few summers in this country have been characterized by greater heat than the one we have just passed through. The excess has prevailed from New Hampshire to Savannah in Georgia. The first general high temperatures of the season occurred on the 3d to the 5th of June, extending from Montreal to Florida, but sparing the west generally. At the south, its maximum, from Chapel Hill, N. C., to Savannah, Ga., was 92°; and at the north, from Montreal to New York, 83°. From the 14th to the 18th, the heat was excessive and general. It commenced at the extreme west on the 12th and 13th, and did not extend beyond Camden, S. C. It rose from 90° to 94° in Ohio, Kentucky, and westward in the same latitudes. From the 20th to the 23d, there was another general excess of temperature—less than the preceding in the extreme north, and with a considerable fall there on the 22d; but quite unusual and long-continued at almost every other part of the country. The maxima varied from the 20th to the 23d, and ranged from 90° to 97°. The maximum of 95°, was probably general from New York to Savannah on the 23d. Lastly, a most extraordinary extreme of heat occurred on the 29th and 30th. The extreme was central in the

[1] Leblond, 184; Pugnet, 342; Rush, iv. 155; Deveze, 117; Emlen, *loc. cit.* 329; Chisholm, i. 294; Nott, Charleston Journ. iii. 5; Fenner, N. O. Journ. v. 203; Caldwell, Repos. vii. 149, 153; Palloni, 33.

[2] On the Climatic Conditions of the Summer of 1853, most directly affecting its sanatory character.—*New York Journal of Med.* Nov. 1853, p. 313, &c.

latitude of Washington, and was limited at Savannah, on the south, and Burlington, Vt., on the north. It attained 96° to 98° in Tennessee, Kentucky, and southern Ohio, and 77.5° to 102° at Washington, and eastern Virginia, and North Carolina. This is without any parallel in the records of temperature at Washington, and is several degrees above any recorded temperature at New Orleans, Mobile, and Savannah.

The temperature of July was also high, and slightly above the normal mean in most parts of the United States. The excessive heat of the last days of June was prolonged through the 1st and 2d of July at 94° in Virginia, at the south, and the range was generally high in this city and south, where it was again at 92° to 84°. The temperature was at or above 90° after the middle of the month only in the central part of Georgia and Alabama, and west in the latitude of Washington to Texas, for two or three days about the 20th, and again about the close of the month.

In August, a period of general excessive heat occurred, beginning, as usual, at the west, and reaching 90° in several places on the 7th and 8th. The maxima in Illinois and the adjacent States were 70° to 90° from the 8th to the 13th; in Ohio and Kentucky, nearly the same; and passing eastward a little later through Pennsylvania, the district of greatest excess was central at New York, from the 12th to the 14th. The temperature at one place south reached 90°. Later in the month, from the 25th to the 31st, the heat was unusually great in the south-west, Texas, the Cherokee Territory, and Mississippi, with an extraordinary reverse in Iowa and the adjoining States.

Indeed, the summer of 1853 has been remarkable for its climatic conditions, and the extreme of temperature was much more striking than usual. Yet, though epidemics of yellow fever, which require a continuance of high temperature, prevailed in more places than has been the case in this country for the last thirty years, many other places that have suffered from the disease on former occasions, as Savannah, Baltimore, and New York, for example, and where the temperature was unusually high, and even exceeded that of Philadelphia, escaped.

In the latter city, too, though the mean temperature of the three summer months was 76.76, or nearly four above the common average, such a heat could not be considered as alone the cause of the disease from which we suffered; for it has been exceeded, ac-

cording to the records of the last sixty-four years, on two occasions —in 1798, when the fever prevailed, and in 1838, when the city was entirely free from it. It must be added that, notwithstanding the unusual heat of the season, we do not find that ordinary autumnal fevers were anywhere more rife, or that they extended over a wider expanse of country than usual; indeed, many localities have remained healthier than in cooler seasons.

Heat, in a word, may be, and is, to a certain extent, requisite to promote the formation of other agencies; it is doubtless essential to farther the evolution of the poison which gives rise to the disease, as everything connected with the appearance of the latter proves—its production in hot weather; its absence in cold, and its disappearance on the accession of frost;—heat may act, besides, as an exciting cause; but alone it cannot occasion the peculiar form of fever under consideration. It requires materials to act upon, and from which, aided by other influences, it may extricate an efficient cause. That high and long-continued heat may, and does often, by its action on individuals unaccustomed to its effects, produce fever, is doubtless true, and perfectly well known to all physicians acquainted with the complaints of hot climates; but the disease thus produced is different from true malarial fevers. More frequently it gives rise to other groups of morbid phenomena more or less distressing, violent, and dangerous—cerebral inflammations, or congestions, visceral inflammations, inflammatory angiotenic fevers; but these phenomena are in no way analogous to those characterizing the regular and specific pyrexiæ under consideration.

Fevers are not the effects of humidity alone.—Neither can we justifiably attribute the effects in question to excess of visible moisture, either of the soil or of the atmosphere, whether produced by rain, or local or accidental causes; or to a high dew-point, or to fogs and dews alone, without the co-operation of some other and more efficient morbific agent. This view of the etiology of autumnal fevers has been advocated by respectable authorities, at home and abroad. Dr. Bell, of this city, informs us that he long had doubts as to the existence or agency of miasma, founded principally on the circumstance that fevers that are usually treated as endemical, not unfrequently become epidemical, and that this extension of such diseases is not pretended to be explained by an extrication of more miasma than usual, but by great irregularities in the seasons,

and abnormal vicissitudes in the weather. The perusal of *Giannini on Fevers*, gave him additional reasons for distrusting the fashionable theory—a result which must appear strange to any one who has seriously examined the work. But when he became acquainted with the valuable experiments and observations of *Dr. Wells on Dew*, and "discovered that all the pretended laws of miasm were, in fact, the phenomena of dew, which latter we could accurately notice, while the separate existence of the former, or its independent action, were never demonstrated," he could not hesitate to abandon his belief in a doctrine not supported by fair induction from observed facts;[1] thus, let it be remarked, arriving, from the same data, at conclusions very different from those which Dr. Wells reached; for that distinguished physician never dreamed of denying the existence and morbific effects of miasm.

The opinion of the main agency of humidity in the production of fever was also warmly supported in England by Fordyce. By this distinguished writer, whose *Dissertations on Fever* are among the most valuable works on the subject in our language, it was admitted that if water is applied in a mass, "that is to say, if a man immerses the whole, or any part of his body in water, of the temperature of the atmosphere, in which he remains some time; or if he throws water of such heat into his stomach," he is not found to be more frequently afterwards affected with fever than other individuals not so exposed; but if the air has particles of water floating in it, thus constituting mist or fog, or otherwise rendering it moist, and a man has continued for some time in such an air, fever very frequently follows. Moisture, therefore, must be the cause of fever. "Some," he remarks, "have contended that the application of water suspended in the atmosphere in the form of moisture, does not produce fever. If those who hold this doctrine "were to live a year or two in Batavia, they would be convinced, by fatal experience, that men living in a moist atmosphere are more frequently affected with fever than a dry one." "Men, wearing any moist covering, have been more frequently affected with fever than those who have worn clothes not moistened with water." Some may escape; but that is no proof of the harmlessness of such exposure; and "the many observations of men being immediately seized with fever, as well as other diseases, after being exposed to moisture, more frequently

[1] Bell on Miasm, 277.

than those who have worn clothes free from all moisture (the other circumstances being the same), which have been both recorded in the annals of medicine, and have come under the author's inspection, give as full evidence that moist clothes are capable of producing fever as any that can be generally procured with regard to the causes of diseases."

"Moisture in the air," Dr. Fordyce continues, "or of the covering of the body, produces more fevers the warmer the atmosphere. But moisture produces fever in all temperatures. The Dutch have endeavoured to make the country of Batavia resemble Holland in the immense number of its canals. The consequent moisture of the atmosphere is very great in both places; but, although fevers therefore frequently occur in Holland, they bear no comparison in number to those which happen in Batavia, where the fatality (owing to the moisture and heat of the climate) is so great that it is wonderful any person should ever approach that settlement, but from the absolute impossibility of otherwise obtaining water or food."

Fordyce was well aware that "when the air is moist, in consequence of water evaporating from a marshy country, or from canals in which the water is stagnating, or moving with a very slow motion, fevers more frequently arise than when the moisture proceeds from the sea, large lakes, or rivers confined within their banks, and running with a considerable degree of rapidity." But this, according to him, is no argument against the correctness of his views; for instances may be cited in which the disease occurred in situations where the decomposition of animal and vegetable substances, such as occurs in marshes, could not be suspected. Thus, in the war which took place in Flanders in the years 1710 and 1711, "an army encamped upon a pure sand, in which water was found in digging less than a foot deep, and occasioned a great moisture in the air, exhibited in a few days numbers of fevers, although the army was perfectly healthy before, and no more fevers were produced on shifting their ground. There are a vast many other instances of the same thing having taken place. Besides, fever has often arisen immediately in persons sitting in rooms the floors of which had just been moistened with pure water. Although, therefore, substances arising from putrefying animal or vegetable matters, in marshes, or other stagnant waters, render the vapour arising from them the more dangerous; yet it does not follow from thence that the particles of the water forming the moisture of the atmosphere,

may not of themselves be the cause of the disease."[1] In his *Fourth Dissertation*, Dr. Fordyce reverts to the same views, and says: "These diseases have been produced in countries where the water was found at only a foot or two under the surface of the earth, whence the moisture has arisen and contaminated the air so as to occasion these diseases, while the soil has been perfectly dry, and there has not been the least appearance of putrefaction, the country being clear from woods. In this case it could be nothing but the moisture that produced the disease. One instance of this occurs in the encampment of the English army in the war about the year 1745, in a sandy plain in Flanders. Another in a region of Peru, where water is everywhere to be found at about seventeen inches below the surface of the earth, though the country itself is barren for the want of water, and uninhabitable from the number of dysenteries and semi-tertians which take place in it."[2]

That fevers frequently occur during rainy seasons, and are, indeed, ordinarily encountered in damp localities, where rain is common and falls abundantly; where the soil, previously dry, has been rendered wet by some of the causes referred to, rain, freshets, overflows, &c.; or where the dew-point is high, and vesicular humidity generally or often noticed, or considerable at the time; that they often make their appearances at the first set in of the rains, the country prior to this having been dry and healthy; and that hence a certain degree of humidity appears to be necessary to the development of the disease—the *wetting*, in contradistinction to the *drying* process, proving injurious to health by exciting the fevers in question—are facts which no one need be told. It is true, also, that in many instances, if not in all, long-continued and thorough terrestrial humidity, or saturation of the soil, has been found to precede epidemic manifestations of yellow and some other forms of malarial fevers; and, on the other hand, that a complete absence of such humidity, a thorough desiccation of the soil to a great depth, as well as an excessive dryness of the atmosphere, are inimical to the production and continued prevalence of those diseases. Facts to that effect have been observed and recorded in this country, in South America, in the West Indies, in Africa, Europe, and Asia. They are true as regards both ordinary autumnal or periodic fevers, and malignant yellow fever. The humidity of the West Indies, of most

[1] Five Dissertations on Fever, i. 76, 79. Am. ed. [2] *Ibid.* Am. ed. 351.

parts of our Southern States and of the coast of Africa, is proverbial; and we know, not only that these are all fever regions, but that the outbreak of the disease coincides often with a wet season, and with the manifestation of increased moisture.

The following remarks of Lind, relative to the African coast, will apply to many other places: "The large rivers in the dry season being confined within narrow bounds, leave a great part of their channels uncovered, which, having their moisture totally exhaled, become a solid hard crust; no sooner do the rains fall than this long-parched crust of earth and clay gradually softens, and the ground, which before had not the least smell, begins to emit a stench, which in four or five weeks becomes exceedingly noisome. At this time, the sickness is generally most violent."[1]

In tropical regions, the sickly or fever season corresponds with that of the rains. In the French colonies, it is denominated *hivernage.* The latter is the period in which the sun heats the portion of the zodiac situated on the side of the equinoxial line where the rains prevail. The *hivernage* is consequently in reality the summer of such sections of tropical regions where it is observed. When the sun crosses the line, the rainy season necessarily changes side; and with the accession of wet weather we have the advent of fever.[2]

The connection of humidity with fever—the necessity of the former for the production of the latter—is exemplified by the occurrences at Tampico in 1836. The rains commence there in July, and are followed by intense heat. This is the period of yellow fever. In the above-mentioned year the rainy season commenced two months later than usual, and there was a corresponding delay in the appearance of the disease.[3]

In Bengal, the rainy season commences in June and continues until October; the remainder of the year is healthy and pleasant. During the rains, the rich and fertile country is almost quite covered by the overflowing of the Ganges, and converted, as it were, into a large pool of water. Diseases rage among the Europeans in the months of July, August, September, and October, consequently during the rainy or wet season.[4]

[1] Hot Climates, 54.

[2] Becquerel, Des Climats et de l'Influence qu' exercent les Sols Boisés, &c. 124.

[3] Goupilleau, Bulletin de l'Acad. i. 456; *Ibid.* iii. 306.

[4] Lind, Hot Climates, 91; Shannon, Practical Obs., &c. on Diseases of Hot Climates, 74; J. Johnson on Tropical Climates, 59.

That the deleterious influence of the atmosphere is aggravated by an undue degree of moisture, has been found on more occasions than one. The following instance is derived from Dr. Home's Dissertation on Remittent Fever. This intelligent physician, who served in Flanders during Marlborough's campaign, and was surgeon to Colonel Cope's dragoons, observed that while the cavalry were cantoned, in 1748, in the unhealthy ground about Bois-le-Duc, the number of the sick corresponded with the dampness of their situation and of the air. To settle the point, he procured a good hygrometer, by which he carefully measured, daily, the degree of moisture or dryness in the air; and, upon comparing his tables with the register kept of the sick, he found that the progress of the disease kept pace, as far, he says, as anything of the kind can do, to the humidity of the air.[1] It is proper to remark that the observations of Dr. Home on this subject were in the malarial districts of the country, and that the greater degrees of moisture, which he found to increase the disease, occurred in the vicinity of marshes.[2]

But experience has shown that this is far from being always the case. Indeed, we find that the reverse often occurs, or, at least, that humidity, whether atmospheric or terrestrial—whether produced by one or other of the sources mentioned, is not, alone and *per se*, sufficient to produce the effect in question. Fevers appear in seasons and times of dryness, when the soil, on the surface at least, is parched for want of rain; when there are no fogs, and little dew. On the other hand, they fail to show themselves under circumstances of an opposite character, and are often arrested in their epidemic course by the very means that are supposed to be by themselves, and without concurrent aid from other conditions of

[1] Dissert. 14; *Ibid.* Med. Facts and Observations, 61, 62; Lind on Seamen, 72.

[2] "From the 29th, O. S., of June, to the 12th, O. S., of July, we had not one man taken bad. During this time, the air was never very moist, though not so dry as what it used to be in my tent during the day-time in camp. So that, taking day and night together, the moisture of my room surpassed the moisture of my tent. On the evening of the 12th, my hydrometer fell very low, and the air was considerably damper than ever I had yet seen it in quarters. From this very night this present distemper began in our regiment; for that night three were seized with it. It continued for eight days damp weather; and the number of those taken bad every day increased. The ten days that followed were drier; during which time not so many were taken bad as before. Two days then followed of damp weather; in which time our number increased. Then, on the weather turning drier, the disease abated. The same equal pace did this disease keep afterwards with the moisture of the air." (Pp. 61, 62.)

atmosphere, instrumental in their production. Major Tulloch, in his excellent Report on the sickness of the British troops in the West Indies, after remarking that the inference of the connection as cause and effect between humidity and disease, derives plausibility from various facts in the history of tropical fevers, especially their great prevalence along the sea-coast, at the outlet of rivers, and in the vicinity of swampy level ground, adds that this hypothesis seems at variance with the facts noticed: "For, if the mortality of the troops depended materially on the influence of moisture, we might expect it to attain its maximum in those stations where the fall of rain was the greatest; whereas, the average mortality of the troops in Jamaica is at least double that which prevails among those in British Guiana, though the quantity of rain which falls in that island is little more than half as great; and in the preceding pages there are adduced many instances in which epidemic fever has broken out, and raged with great violence, at a period when no rain had fallen for several months; nay, in some stations a dry, in others a wet season, is looked on as the most unhealthy —an anomaly not likely to occur if excess of moisture was uniformly an essential cause of insalubrity."[1]

Madeira, the Canary Islands—not far from the coast of Africa—the islands of St. Antonio and St. Nicholas, are healthy, though humid; while Fernando Po, Princes, and St. Thomas islands, not far from these, are, like Senegal, unhealthy. Barbadoes, St. Christopher, and Bermuda, though at no great distance from fever countries, are healthy; and, while St. Lucia is unhealthy to a degree, Pigeon Island, which is not less humid than the opposite coast, is free from febrile diseases. Dr. Rollo calls attention to the circumstance, that some of the troops that landed in 1778 at St. Lucia, and were encamped at the Vigie, were there exposed to fatigue, *constant rain*, and changes from heat to cold, and yet they were not sickly. They were thence removed to the windward of the Carenage, where, to the former causes, were added marshy exhalations. They then became subject to fevers, from which they were once more freed by resuming their former position.[2]

Heberden many years since remarked that the air is often fully saturated with moisture, and could not be more filled by the vapour arising from a chamber covered with water; and yet, he adds,

[1] P. 101.

[2] Diseases of the Army at St. Lucia, 67.

"neither is any epidemical distemper produced by it, nor are those remarkably aggravated with which the sick happen at the time to be afflicted. The air from rivers and from the sea, is probably more replenished with vapours than inland countries cleared of their woods, yet they are generally healthier."[1] Dr. Ferguson has shown, from undeniable facts, that mere humidity from fresh water is not productive of fever.[2] He remarks that water kept in stone tanks, or anywhere, so that it can be preserved in bulk without being absorbed by the surrounding soil, is not productive of disease. One of the healthiest quarters in the West Indies, according to that able physician, is that of the field officers on Berkshire Hill, St. Vincent, the bedroom of which is placed immediately over a deep stone reservoir of water. A block-house in Demerara, reported to be one of the healthiest quarters there, is similarly situated; and it is known to all that the fresh water laid in for a ship's crew, however much in contact with their sleeping-places, produces nothing like marsh fever amongst them. Similar statements relative to the innocuousness of fresh water will be found in the works of McLean,[3] Lempriere,[4] Bancroft,[5] Belcher,[6] R. Jackson,[7] Pugnet,[8] Dickson,[9] Drake, and others.[10]

In speaking of Fort Augusta and Port Royal, Jamaica, Dr. Hunter remarks that simple moisture is harmless, "at least as far as relates to the production of fevers, of which the two last-mentioned places may be given as examples, for they are nearly surrounded with water on all sides. It is true," he adds, "the air is perfectly clear, yet it must be loaded with moisture in consequence of the great heat of the sun acting upon the water. But the vapour arising from water is harmless, even when rendered more an object of our senses, by being condensed into fogs and clouds. The parish of St. Thomas in the Vale, is every night covered with a thick fog,

[1] Med. Tr. by the College of Physicians of London, ii. 523–4.

[2] Med. Ch. Tr. viii. 129.

[3] An Inquiry into the Nature and Causes of the great Mortality among the Troops at St. Domingo. London, 1797; 24–25.

[4] Practical Observations on the Diseases of the Army in Jamaica in 1792–1797, ii. 5, 6.

[5] An Essay on the Disease called Yellow Fever, 243.

[6] Edinb. Med. and Surg. Journal, xxiii. 47.

[7] A Sketch of the Hist. and Cure of Febrile Diseases, 11.

[8] Memoire sur les Fièvres de Mauvais Caractère, 342–3.

[9] New York Med. Journal, Sept. 1841; 175.

[10] Diseases of the Valley of the Mississippi, &c. i. 610, 711.

owing to the rivers which pass through it sending forth vapours, which in daytime are perfectly transparent, but towards evening, by the cool air coming from the neighbouring mountains, they are condensed, and remain visible till next day's sun disperses them, without, however, being at all unwholesome."[1] Other facts lead to the same conclusions. In an excellent report on the epidemics of France during the years 1841–1846, by M. Gaultier de Claubry, mention is made of a small commune—St. Jean de Losne—situate in a valley of the Department of Côte d'Or, along the banks of the Saone, and near the canal which unites this river to the Rhine. This commune, from its position, is much exposed to fogs, and remarkable for its great humidity. Nevertheless, it is usually free from periodic diseases. In 1843, however, it was visited by an epidemic of intermittent fever, which was easily traced to other causes, and could not be due to the hygrometrical condition of the atmosphere, which at other periods failed to produce that effect.[2]

M. Madier, who practised at St. Andéol, a small town in France, says of fogs: "We are exposed to them at all times, especially during the spring and autumn. The fogs, which show themselves after a few days of dry weather, and when a light rain has fallen the day before, have a very strong earthy smell; but when they appear after a rain which has penetrated the soil to the depth of a few inches, they do not emit any odour. I have not noticed that fogs, in whatever season they may prevail, have exercised an injurious effect on individuals in health, and given rise to any particular disease."[3] Let it be remarked that the country around this town is not marshy.

If fogs and heavy dews were of themselves, and without the concurrent aid of other and more efficient agencies, the active instruments in the production of fevers, there would be no difference whence they came. The effect would be the same whether they arose in elevated and mountainous regions or in valleys and plains. Fever would be noticed in all moist and foggy countries—in the Western Highlands or Cornwall, more than in Norfolkshire or Lincolnshire, as also in every place where dews are heavy; and, on the other hand, localities, where fogs and dews are not observed,

[1] Obs. on the Dis. of the Army in Jamaica, 13, 14.

[2] Mem. de l'Acad. de Médecine, xiv. 120.

[3] Mem. sur la Topographie Méd. du Bourg St. Andéol; Mem. de la Soc. Royale, v. 78.

would not be the seat of febrile complaints. Now, we know that this is not the case. These atmospheric conditions are not the products of malarial localities only. Dew, for example, will be abundant wherever vegetation is rich, and may, like fogs, be seen as well on hill-tops, where no malaria exists, as on low ground, where it is copiously evolved; or both may prevail in a place during healthy seasons, and be less or not more observed during sickly ones. This is no fancy on my part; for if the reader inquires into the matter, he will easily find that everywhere localities present themselves, which, like those referred to by Dr. Hunter, are subject to fogs and heavy dews, and yet notwithstanding are healthy; while, in numerous instances, fever has broken out under meteorological conditions which forbid the possibility of its being referred to such agencies. Thus, the banks of some streams in this country, and not a few at a short distance from our city, though the seat of heavy fogs and dews, are seldom visited by autumnal fevers, not as frequently so, certainly, as other kindred localities differently circumstanced. Such is the case with the upper part of the Wissahicon, where, notwithstanding the fogs and dew observed in certain seasons, malarial diseases are not known. The city of Charleston furnishes us with an interesting illustration of the want of connection between the degree of evaporation and condensation and the production of malarial fevers. During July, August, and September, of the fever year of 1849, the evaporation was much higher than it was in the non-malarial years of a series extending from 1845 to 1852 inclusive, amounting to 1,485. But in 1852, which was highly malarial, the evaporation did not exceed 1,454, while in 1850 it was 1,418; a difference too trifling to justify us in investing it with importance. As to the degree of condensation, it amounted, in 1849, to 173; contrasting with what took place during the two non-malarial years of 1850 and 1851, when it was 111 and 81. On the other hand, the sickly year of 1852 gives us an amount of only 83; only two degrees higher than 1850, and 28 lower than 1851.[1]

Were fogs and mists the efficient and active cause of fever, we might expect to find the disease prevailing in other seasons besides that in which it almost exclusively shows itself; and yet in regions visited by it, the humidity produced by, or connected with, the thaws and fogs of the spring, and which, in many instances, is as

[1] Hume, Charleston Journal, viii. 67.

considerable as, if not even more so than, at the sickly period, proves perfectly innocuous, heat being often at the time considerable.

The waters of the Gulf Stream flow with great rapidity near the banks of Newfoundland, bringing with them a temperature of from six to twelve degrees warmer than that of the superincumbent atmosphere, and of the sea itself in that part of the ocean according to the season of the year. This superior heat in the Gulf Stream, as is remarked by Dr. Bancroft, aided by its motion, produces a copious evaporation of aqueous particles from the surface, which are immediately condensed by the coldness of the air, so as to produce those fogs which, during summer, prevail on the Newfoundland station, to a greater excess, probably, than in any other part of the globe.[1] Mr. Cassini, as quoted by Bancroft, states, in the account he has given of his voyage to Newfoundland, that "it is difficult for one who has never been there, to form an idea of the life the fishermen lead at the Great Bank. It must be no less powerful a motive than the thirst after gain which can prevail upon these poor wretches to spend six months between the sky and water, in a climate where they are almost always excluded from the sight of the sun, and constantly breathing so thick a fog that they can hardly see from one end of the ship to the other." Yet, notwithstanding, the men so employed are remarkably healthy and free from fever. "The island," says Major Tulloch, "has been long noted for the frequent and dense fogs which prevail along its banks, and often continue during a great part of the summer. None of these agencies, however, seem to operate prejudicially on the health of the inhabitants, among whom the mortality is on a lower scale than on any portion of the American continent.[2]

When, with these facts before us, we bear in mind that fogs, vapours, and dews are only injurious—so far as the production of fever is concerned—in localities that are marshy or swampy, or that possess a soil composed of materials capable of furnishing malarial exhalations—in other words, in situations where fevers often prevail without the aid of such hygrometrical phenomena, or in those open to the free admission and active agency of winds passing, before reaching them, over surfaces of that kind, as is the case with the smokes of the African coast; we cannot resist the conclu-

[1] Essay on Yellow Fever, 189, note.

[2] Report on Sickness of Troops in British America, 35–6; Lind on Hot Cl. 30–1.

sion that, when such fogs, vapours, and dews exist in sickly districts, they act merely as predisposing or exciting causes, and as vehicles of the poisonous agent, and should not be regarded in the light of the efficient cause of the disease.

In addition, it may be remarked that, in the large majority of places subject to fever, the disease makes its appearance not at the height of the rainy season, when the moisture of the atmosphere must be greatest, but at the commencement and close of it, when the soil is moistened without being drenched, or after it has become somewhat dried. Nor should we forget that vessels at a short distance from the coast remain healthy, and take the fever by approaching near, though the humidity in the former situation was, if anything, greater than in the latter; that some forms of fever are more prevalent and assume a more malignant character during dry weather, and even during droughts; that in yellow fever regions, seasons marked by much rain have often proved the healthiest; that, in some localities, in proportion as the humidity of the climate has lessened, fever manifestations have increased; and that the disease is sometimes checked by heavy rains.

We have here, therefore, the reverse of what was said above of the injurious effects of the *wetting* process; for while the latter, by its operations on localities more or less dry and parched, gives rise to fever, the same disease is often an attendant on, and a result of, the *drying* process;—not making its appearance until the surface, after having been covered or saturated with water, is becoming, through the evaporating agency of heat, to a considerable extent desiccated; excess of dryness and excess of moisture being alike inimical to the production of the febrile cause. In neither of these two processes can we admit, therefore, a monopoly in regard to the power of giving rise to that dire effect; and the very fact of two influences of such antagonistic characters exercising that power, under opposite local circumstances, must naturally lead to the inference that the result in question is obtained, not in virtue of any morbific influence dependent on a wetting or drying operation, abstractly considered, but on the change they each occasion in the soil of the locality thereby rendered sickly, or in the organic materials placed on its surface;—drying them in the one case when too wet, and moistening them in the other when too dry. If we admit this, we must admit also that as the effect is obtained in none but localities of the peculiar kind already mentioned, and as neither

the wetting nor the drying process exercises any morbific influence of the kind alluded to, under different conditions of soil or locality, that effect, when produced, must be due, not to a little more or a little less dryness, either of which, as we have seen, may exist in salubrious situations, but to the extrication from that soil or the materials scattered over it, of some peculiar agent which operates as a poison on individuals exposed to its influence.

But let this be as it may for the present, with the above facts before us, we can better appreciate the pertinency of Major Tullock's remarks, already referred to, that "in some stations a dry, in others a wet season is looked on as the most unhealthy." Long before Major Tullock's days, Dazille had recorded the same observation in a passage strangely mistranslated by Dr. Rush, and handed down, in all its imperfections, by several successive writers. "It is during dry weather that diseases prevail at Cayenne; on the contrary, it is during the rainy season that they spread in St. Domingo. The reason of this difference is that at Cayenne, during the period of rains, the marshes contain a sufficient quantity of water to be preserved from corruption, and to be renewed gradually by the flow and ebb of the sea. When once the rainy season is over, the waters become stagnant and in a state of corruption, and occasion by their putrefaction that of a large quantity of insects and animalcules, the effluvia from which are exhaled in the atmosphere, thence pass, through means of respiration, into the lungs, and carry into the humours the germ of the diseases which afflict the inhabitants of the vicinity."[1] An accurate observer of modern times states, in reference to the malarial fevers of Europe: "The most unhealthy years, in humid districts, are those noted for excessive heat, or great and long-continued dryness; while in dry localities, the most sickly are the rainy years."[2]

The following facts illustrate the injurious effects of the drying process:—

In India, during the unwholesome season, as stated by Bishop Heber, in the account of his journey through that country, all living things leave the pestiferous region, and man dare not venture abroad. "Yet, during the time of the heaviest rains, while the water falls in torrents, and the cloudy sky tends to prevent evaporation from the ground, the forest may be passed with tolerable safety. It

[1] Dazille, Maladies des Nègres, 10. [2] Villermé, Annales d'Hygiène, xi. 347.

is in the extreme heat, and immediately after the rains have ceased, in May, the latter end of August, and the early part of September, that it is most deadly." Of another pestiferous locality an intelligent writer remarks: "The different seasons of the year have considerable influence upon the unhealthiness of Batavia. On this subject erroneous notions have been laid before the public. The rainy season, during the months of January, February, March, and April, has generally been represented as the most unhealthy. This is owing to an error in observation. During the rainy season, the rivers and canals are plentifully supplied with water, which flows through them with considerable rapidity; most of the lower marshy situations are entirely inundated with water, by which the existing putrefaction is either very much checked or entirely prevented." In June, the marshes and canals begin to dry up, and with this the unhealthiness commences. In July and August, the dryness and exposure of the latter are still greater, and the disease is rife. "This is the season of death and destruction." In October, the dryness, etc., being more excessive, and the materials of decomposition being more thoroughly destroyed, the disease decreases and becomes less malignant. In November and December the effect is still greater.[1]

According to the observations made by M. Don, chief civil engineer at Algiers, as quoted by M. Becquerel, from the *Annuaire Metéorologique de la France* (1850), and embracing a period of ten years, from 1837 to 1847, rain in that country increases from August to December, and decreases from January to July; the maximum being in December (175.444 millimetres), and the minimum in July (0.150 millimetres). The dryness commences in May, and continues to the end of September, sometimes to October.

The 1st	quarter	from	1st December	to 28th February,	451.594
" 2d	"	"	1st March	to 31st May,	211.380
" 3d	"	"	1st June	to 31st August,	015.175
" 4th	"	"	1st September	to 30th November,	261.088

From this, we perceive that one quarter is very wet, that another quarter is very dry, and that these are separated by two quarters differing but little from each other in respect to the amount of rain.[2]

[1] Horsefield, An Account of a Voyage to Batavia in the Year 1800, Coxe's Med. Mus. v. 78.

[2] Becquerel, Des Climates, &c. 174, 175.

Dr. Haspel,[1] whose observations were made in the province of Oran, divides the year into four periods. The first commences in March and ends in June. At this period but little rain falls. The second embraces July, August, and September. It is characterized by much dryness. The third period commences in October and ends during the course of December. Rain now commences, and falls in showers, which, with the torrents flowing from the Atlas, submerge the plains. The fourth period comprises the latter half of December, the whole of January and February, and the beginning of March. It is characterized by much rain, which sometimes commences at the close of November, and recurs at longer or shorter intervals till the close of the following April.

Now, on referring to the highly interesting work just mentioned, and to others on the same subject, it will be found that at Algiers, as in other kindred climates, the spring months are healthy; while the dry and parching weather of July, August, and September is extremely unhealthy. Fevers then abound, assume the remittent type, and become often pseudo-continued, and malignant. In the third period, which, as we have seen, is wet, malarial fevers, though becoming more severe and complicated, lessen in frequency. During the fourth and still wetter period they disappear nearly if not completely; showing themselves only in individuals who have been attacked in the preceding summer or autumn.

Of the years 1845 to 1852 inclusive, the last and 1849 were the only ones during which the yellow fever prevailed at Charleston. In 1852, the quantity of rain which fell during July amounted to 6.95 inches; August furnished 4.21, and September 12.27; making a total of 23.43. In the year 1849, July gave 6.35, August 5.16, and September 6.27; total 17.78. But while the quantity in the first of these two sickly seasons exceeded considerably several of the healthy years of the series, it was only 0.68 greater than the quantity in 1847, when no fever prevailed; while the amount that year (22.75) and in 1845 (19.71) exceeded that in 1849 by several inches. So far as Charleston is concerned, therefore, though, as remarked by Dr. Hume, from whom these facts are derived, the yellow fever has never appeared in a non-pluvial season, showing that some degree of humidity is required; yet the facts mentioned, and many others that could be gathered from Dr. Chalmers's account of

[1] Maladies de l'Algerie, i. 28.

the climate of South Carolina, pluvial seasons are not necessarily accompanied by fever.[1] In 1752, 27.45 inches fell during July, August, and September; and yet, notwithstanding, the season was healthy.

In Louisiana, while some of the most humid and wet localities are little affected by intermittents, others, differently circumstanced in that respect, are highly subject to the disease. Such is the case in East Feliciana, and the parishes lying to the east of it. "These parishes consist of high lands, which constitute a portion of that bluff-formation of the south, and have an elevation of from one hundred to two hundred and fifty feet above the level of the Mississippi River at that point. The lands are generally thin, and covered either with open forests of the long-leaf pine, or with those of oak, beech, and other trees, intermixed with a growth of loblolly pine, and other species." "In this region, the climate is obviously drier than in the low lands of the delta, as is shown by hygrometrical tables, as well as by the fact that here the Spanish moss is scarcely met with, while in the low lands it covers every tree, and the growth of this plant is a good hygrometrical index. Notwithstanding the favourable aspect of these regions, as respects health, the inhabitants are very subject to ague and fever."

This connection of dryness with unhealthiness had not escaped the notice of Lord Bacon, who remarks: "The general opinion is, that years hot and moist are most pestilent; upon the superficial ground that heat and moisture cause putrefaction. In England, it is found not true; for many times there have been good plagues in dry years. Whereas, the cause may be, for that drought, in the bodies of islanders habituated to moist airs, doth exasperate the humours and maketh them more apt to putrefy or inflame; besides, it tainteth the waters commonly, and maketh them less wholesome. And again, in Barbary, the plagues break up in the summer when the weather is hot and dry."[3]

The fever which prevailed at Copenhagen, in 1652, and is described by Bartholin, began in autumn, after an unusually hot and dry summer.[4] In 1669, Leyden was visited by a like fever. The spring and beginning of summer were cold, but the remainder

[1] Hume on Causes of Yellow Fever, Charleston Journ. viii. 64.

[2] Carpenter, New Orleans Journ. iii. 429.

[3] Bacon, Nat. Hist. Cent. iv. Exper. 383.

[4] Hist. Anatomia, Rar. Cent. ii. hist. 56.

of summer and the autumn were unusually hot, with little or no rain.[1] "It is remarkable," says Pringle, "that pestilential diseases have frequently occurred in dry and hot summers, and agreeably to this, I have observed that most sickly seasons in the field have been attended with the greatest heat and the least rain."[2] Hippocrates himself has left his testimony on this subject, for he mentions remittent fevers as having been common in summer and autumn, and most prevalent when wet springs with southerly winds were succeeded by hot and close summers.[3]

The example of Sicily is apposite. Light rains in autumn are there unhealthy, evidently from their moistening the earth, which previously had been dried effectually and to a great depth by the heat of summer, and thereby putting it in a state capable of evolving febrific effluvia. Whereas, nothing is as salutary as *heavy* rains about the middle of September, which not only mitigate the heat, but, by covering or saturating the earth, put a stop to the production of the cause of fever.[4] The country is penetrated in several directions by ridges of primitive hills of considerable height, and apparently a continuation of the Apennines. Between these are numerous watercourses, which are dry in summer, and occasionally filled by torrents in winter. They are denominated *fiumari*, by the natives, and are used as roads in the dry season. Many of them are extremely unhealthy in the latter part of summer and in autumn, and infected by malaria. Much of this unhealthiness, however, depends on the state of the season. A very dry season will, while parching the surface of the earth, put an obstacle to the production of the cause of fever, and render the country healthy. The same effect is produced by a very wet season, which, by saturating the earth and completely filling up the *fiumari*, and covering their sickly beds, puts a stop to the extrication of the febrific cause. Fever there is rife only at periods when the soil is partially saturated, and principally while the drying process is going on, or when, after having been parched and completely dried, it is moistened by moderate rains. In situations where, from peculiar local circumstances, complete desiccation does not occur, and the ground is only

[1] Silvius de la Boe Prax. Med. Append. tract. x. Oratio de affectus epidemici Leidensis causis naturalibus dicta. Leyd. 1670. 12mo.

[2] Dis. of the Army, 81, part 2, chap. ii.

[3] Epidemics, lib. iii. Sect. 3.

[4] Irvine, Observ. upon Diseases of Sicily, 2.

partly freed from water, as at Lentini, around which the country is marshy, with a considerable lake in the vicinity, or, among other places in the large *fiumare* which bounds Messina on the northern side, where the stream disappears in the gravel, and, percolating under the surface to the sea, fresh water is found at a foot depth, fever prevails every year, and at all seasons.[1] In Sardinia, too, fever rages from June to September. In some summers there is a want of rain for four or five months, and then it is that sickness exerts its utmost violence.[2]

Senegal is proverbial for the wide and extensive prevalence of periodic fevers. There, as in many other places, the country is healthy during the height of the rainy season, when the soil, like that of Egypt, is nearly all covered over with water, and the moisture of the atmosphere is necessarily considerable. During the drying process which succeeds to this condition of things, and commences about the middle of September, the soil gradually becomes uncovered, and soon adorned with the richest vegetation; while in its moist state, it remains for some time exposed to the influence of the burning sun. This is the period of fevers. These extend far and wide, and continue to prevail until, through the effects of heat and other agencies, the desiccation of the soil and the dryness of the atmosphere become complete. As soon as these results are obtained, the country once more becomes healthy.[3]

In several of the fluvial basins within the torrid zone, or on its margins, the rainy season is the period of health and the dry season that of disease. See what takes place in the district of Lower Egypt. There the dry season, when the Nile is low, and the country is moistened solely by irrigation, is febriferous. In May and June the majestic river begins to rise, and the increase of the water, and consequently of the humidity of the soil, is hailed as the sign of approaching salubrity.[4]

[1] Irvine, Observ. upon Diseases of Sicily, 3, 4, 5.

[2] Lind, 31. [3] Thevenot, *op. cit.* 21, 25, 48.

[4] Prosp. Alpines, De Morb. Ægypt, lib. i. cap. xiv.; Craigie, Pract. i. 81; Volney, Voy. en Egypt, i. 213; Macculloch on Malaria, 107; Sir James McGrigor, Sketches, 1.

The researches of M. Villermé on the influence of marshes on the duration of life,[1] have led to interesting conclusions, a summary of which may be appropriately placed here. In the eight most marshy departments of France (Ain, Charente Inférieure, Gard, Gironde, Hérault, Bouches du Rhone, Var, and Vendée), where epidemics of

[1] An. d'Hyg. xi. 346, &c.

We have seen that, in Bengal, the sickly is the rainy season, and that fevers are rife when the country is wet. It appears, however, that the moisture must keep within certain bounds. It is remarked that the more complete the inundation, the more healthy are the inhabitants, till the fall of the waters, in November and December, expose a number of miry and slimy marshes to the action of a still powerful sun, when those who are in their neighbourhood are sure to come in for a share of remittents and intermittents.[1] The same effects that are noticed in Bengalese, after the waters begin to fall, take place when the inundation has been considerable enough to cover the country. "When the rains are late•

periodic fever reign annually during two, three, or four months, the mortality among children under four years of age is much greater in August, September, and October, when fever prevails, than in January, February, and March; while the whole number of deaths among individuals, from four years old to one hundred, always attains its maximum in winter; in other words, the influence of marshes on young children is such, supposing the effect to be produced by one cause only, that in every thousand deaths of young children which take place in healthy districts, 1,546 occur in the aforesaid eight departments. The baneful influence of marshes in the above-mentioned classes will appear still more striking, if we compare the mortality which occurs in paludal districts during the spring or the commencement of summer, when it is least, with that which occurs during August, September, and October, when it is largest; for it will be found that for every death noted in the first period, we have in each of the three other mentioned months, according to the localities, two, three, four, sometimes five, and even six. In the aforesaid eight most marshy departments, the number of deaths occasioned by paludal exhalations, among very young children, is greater in September; while the mortality from the same cause among other individuals, reaches its maximum in October. But although, as we have seen, the loss among children in those eight departments, considered together, is largest in September, we find that, in the departments of Ain and la Vendée, the least southern of the series, and those in which the desiccation of the marshes is effected at a later period, the most fatal month is October. In the other departments, the period of that maximum is earlier in the ratio of their southerly situation, or in proportion to the earliness of the desiccation of the marshes. Indeed, the period at which these are dry, as well as that of the prevalence of the disease, and of the large mortality they occasion, advances in the south of our hemisphere, and is retarded in the north. When the march of the seasons advances, or is retarded; when the drying up of the marshes is longer in being effected, or is accomplished with greater rapidity, the greater mortality they produce commences earlier or later, and continues in the autumn long after the hot weather, or ceases during the continuance of them. M. Villermé properly remarks that, as the maximum of the mortality among children in the marshy departments, and an increased mortality at other periods of life, constantly coincides with the period of desiccation, complete or partial, of the marshes, and not with that of the greatest heat, it follows that this effect must be ascribed more to that desiccation than to a high degree of temperature.

[1] Johnson on Tropical Climates, 43.

in setting in, many people are suddenly cut off by the intense heat of the sun in June and July. But this is nothing compared to the havoc produced by a sudden and premature cessation of the rains, or Bursautty, as they are called. In this last ease, an immense surface of slime and feculence is all at once exposed to the rays of a vertical sun." "The consequence is that the profuse exhalation of miasmata spreads pestilence in every direction."[1]

The air on the flat country which stretches along the eastern and northern coast of the island of Ceylon, is very dry and hot, during the south-west monsoon. Showers rarely occur there during the months of May and October. During the influence of this monsoon, a hot land wind blows from the interior towards the eastern and northern coast of the island. This wind sets in about the middle of May, and blows, with but little intermission, till the end of August. In the other months of the year, there are regular sea and land breezes. While the land wind prevails, there are but rarely any sea breezes. The land wind often blows day and night for several weeks together, without much abatement; it is always very dry and hot. Dr. Marshall, from whose work on the medical topography of the interior of that country I quote, states, in another page, that the above provinces are remarkable for insalubrity. The late king of Kandy sometimes took advantage of the pestilential atmosphere of these districts, and transported thither the chiefs whom he considered disaffected to his interest. Few of the inhabitants escape the influence of the fever during the sickly season (pp. 6, 40). Dr. John Davy, in like manner, informs us, that fevers are rife in Ceylon during the season of dry weather (p. 75). Dr. Rollo mentions that the greater part of the regular remittents that prevailed at St. Lucia did so during the rains, when the pools and marshes were filled; and that the most dangerous remittents appeared after their slimy surfaces became exposed and dry.[2] Dr. Copland states that the ditch around the ramparts of Geneva was once drained, and sickness prevailed in the vicinity, but disappeared when it was again filled.[3]

Dr. W. Ferguson, whose name has become indissolubly connected with the subject of miasma, and who, though erring in pushing his notions as to the necessity of a dried surface farther than is war-

[1] Johnson on Tropical Climates, 44. [2] *Op. cit.* 69, 70.
[3] Dict. i. 763, Endemic Diseases.

ranted by a survey of all the facts we possess, and ignoring the agency of the decomposition of organic substances (to say nothing of the inconsistency of denying in one place this agency, and in another referring the fever which occurred on board of the Regalia, to the decomposition of a quantity of green wood stowed away in the hold of the vessel)—has added considerably to the stock of our knowledge in relation to the sources of that poison, and furnished many facts corroborating the views here advocated. The English army suffered much from endemic fever, under the remittent and intermittent forms, at Rosendaal and Oosterhout, in Holland, in 1799. The summer had been hot and dry, and the soil, which was a level plain of sand, presented a perfectly dry surface. But on digging, it was universally found to be percolated with water to within a few inches of the surface. In 1799, the army, under the Duke of York, remained the whole autumn in the most pestiferous portion of that unhealthy country without suffering in any remarkable degree from fever. But the summer had been wet and cold. It rained constantly, and the whole country was nearly flooded with water. In 1810, the British army at Walcheren, on a soil as similar as possible, and not more pestiferous, suffered considerably from fever. But the soil, after having been thoroughly wet, had passed the ordeal of a hot and dry summer, by which the surface was partially desiccated. In Portugal and Spain, during the campaign of 1809, Dr. Ferguson found the hilly ravines that had lately been watercourses, but were now dried up by a continuance of several weeks of very hot weather, to be very unhealthy. The same results obtained during the retreat into the plains of Estremadura, along the course of the Guadiana River (after the battle of Talavera). The country, which we may presume *had been* wet, was then arid and dry. The river itself, and all the smaller streams, had, in fact, ceased to be streams, and were no more than lines of detached pools, in the courses that had formerly been rivers. The Alentejo land, opposite Lisbon, is dry, superficially flat and sandy, and very unhealthy. So also is Salvatora, a large village about a mile inland. "The country around is perfectly open, though very low, and flooded with water during the whole of the rainy season; but at the time of the periodical sickness, it is always most distressingly dry." "Cividad Rodrigo affords another illustration of the same. It is situated on a rocky bank of the River Agueda, a remarkably clear stream; but the approach to it, on the side of Portugal, is

through a bare, hollow country, that has been likened to the dried-up bed of an extensive lake; and upon more than one occasion, when this low land, after having been flooded in the rainy season, has become as dry as a brick-ground, with the vegetation utterly burned up, there arose fevers to our troops, which, for malignity of type, could only be matched by those before mentioned on the Guadiana." Corea, in Spanish Estremadura, on the banks of the Alagon, is highly unhealthy. Yet at the time the British troops suffered there, the shores of the river seemed perfectly dry, and there was not an aquatic weed, nor a speck, nor a line of marsh to be seen within miles of the town, nor anything but dry, bare, and clear savanna. From the foregoing facts, as Dr. Ferguson remarks, it will be seen that, in the most unhealthy parts of Spain, we may in vain, towards the close of the summer, look for lakes, marshes, ditches, or pools. Spain, generally speaking, is then, though as prolific of endemic fever as Walcheren, beyond all doubt, one of the driest countries in Europe, and it is not till it has again been made one of the wettest, by the periodical rains, that it can be called healthy or habitable with any degree of safety.

Dr. Ferguson's observations in the West Indies led him to the same conclusions. "It might there be seen, that the same deep, marshy country which the rains made perfectly healthy, as if by deluging a dry well, was speedily converted, under the drying process of a vertical sun, into a hotbed of pestiferous miasmata. Thus, in the Island of St. Lucia, the most unwholesome town of Castries, at the bottom of the Carenage, which is altogether embosomed in a deep mangrove fern, became perfectly healthy under the periodic rains; while the garrison, on the hill of Morne Fortuné, immediately above it, within half cannon-shot, began to be affected with remittent fever. The two localities within this short distance evidently changed places in respect to health. The top and shoulders of the hill had been cleared of wood, and during a continuance of dry weather, the garrison had no source of disease within itself; but this was amply, though but temporarily supplied, as soon as the rains had saturated the soil on which it stood. Thus, an uncommonly rainy season at Barbadoes seldom failed, in that perfectly dry and well-cleared country, to induce for a time general sickness; while at Trinidad, which is almost all swampy, and the centre of the island may be called a sea of swamp, where it always rains at least nine months of the year—if it only rained eight, or

if at any time there was a cessation of the preserving rains, the worst kind of remittent fevers were sure to make their appearance. General dryness of soil, however, is far from being the ordinary characteristic of our West India colonies. The swamp is too often exposed to the continued operation of a tropical sun, and its approach to dryness is the harbinger of disease and death to the inhabitants of its vicinity. On the whole, it may truly be said, that although excessive rains will evidently cause the acknowledged wholesome and unwholesome soils to change places for a time, in respect to health, a year of stunted vegetation, through dry seasons, and uncommon drought, is infallibly a year of pestilence to the greater part of the West India Colonies."[1] In all cases, however, previous saturation of the soil is necessary to insure the effect, and fever ceases when the exsiccation is thorough and complete.

On all these points, other facts, numerous and authentic, may be found in other writers on the fevers of Europe, of the West Indies, of Mexico, South America, Africa, and this country.[2] Indeed, we can scarcely open a book, large or small, which treats of the subject in *extenso* or incidentally, without finding therein a repetition of the same story—absence of fever during excess of terrestrial humidity;

[1] Marsh Poison; see Notes and Recol. of a Professional Life, 186, 198.

[2] Baglivi, 157–9; Ramazzini, Opera Omnia, London, 1717; Rochoux, 11; Berthe, 51, 156; Pariset, 177; Fellowes, 13, 32, 35; Boyle, Ed. J. viii. 178; Beam, Tr. of Med. Soc. Lond. v. 335; Report on Gibraltar Fever, 4; Caillot, 121; Furlong, Med.-Chir. Rev. xxv. 290; Valentin, 87, 89; Lempriere, i. 26, 31; ii. 47, 48, 49; Desportes, i. 52, 80, 86, 87; Ferguson, Med.-Ch. Tr. viii. 130–1; H. M'Lean, 25, 72; Gillespie, 20, 137; Johnson, Trop. Cl. 362; Dazille, Mal. des Negres, 10; Firmin, Mal. de Surinam, 3, 18; Bancroft, 200, 314; Edinb. J. lxiii. 448; *Ibid.*, lxix. 132; Bally, 304, 363; Hillary, 146–7; Jackson, Outlines, 92; Tullock's Rep. 64; Gilbert, 4, 50, 69; Rufz, 10, 29; Catel, 9; Beguerie, 10; J. Clark, 75; Towne, 8; Leblond, 229; Imray, Ed. J. liii. 93; lxiv. 331; Pinkard, ii. 486; Henderson, 8; Arnold, 31, 148, 174; Chisholm, i. 145, 294; ii. 196; Humboldt, 765; Boussingault, An. de Chimie, lvii. 151; Boteler (Voy. of Disc. by Capt.), i. 137, 155, 156, 356; Pallas, 109; Pritchett, 266; M'Williams, 176; Boyle, 6, 44, 123; Brit. and For. Med.-Chir. Rev. i. 382; Jacquot, 14; Simon's Rept. to Bd. of H. of Charleston, 6, 10, 18; Watts, Med. Reg. of N. Y. 278; Barton, 13; Revue Med., 1840, 322; Merrill, Chapman J. ix. 233, 240; *Ibid.* N. O. J. viii. 7; *Ibid.* N. A. J. ii. 218; Rush, iv. 154; Chabert, 20; Lining, ii. 407; Townsend, 263; Barton's J. ii. 22; Drake, 717; Gros, 5; Johnson, Charleston J. iv. 155; Hulse, Maryland J., Jan. 1841, 392; Thomas, 18, 63, 213; Pendleton, Charleston J. vii. 449; Merrill, Address on Health and Mortality of Memphis, Recorder, i. 88; Bonnet, Tr. des Fièvres Interm. 304; Craigie, Pract. of Med. i. 82; ii. 171; Villermé, An. d'Hyg. xi. 349, 350; Goupilleau, Bulletin de l'Acad. i. 456; iii. 306; Sigand, Du Climat et des Mal. du Brezil, 70–2.

occurrence of the disease under the influence of the drying process; again, absence of fever when the exsiccation of the soil is complete, and penetrates to a considerable depth, and every particle of moisture has been destroyed; and afterward reappearance of disease during the wetting period.

It must be remarked besides, and more especially in reference to the facts adduced by Dr. Fordyce, in support of the power of moisture in producing fever, that most of the instances in which the disease is supposed to have arisen from that cause, occurred in situations more or less characterized by a marshy condition of the soil. Of all known localities, Batavia is the last that should have been selected as illustrative of the febrific agency of simple moisture. "It is impossible," says our countryman, Dr. Horsefield, who was well acquainted with the country, "for the imagination to conceive a situation more favourable to the production of marsh miasmata than that of Batavia." "If human industry and ingenuity should be exerted in planning and constructing a laboratory for the production of pestilential vapours, a situation exactly resembling that of Batavia and its environs would be the result."[1] The same may be said of many other fever districts where moisture is sufficiently considerable to be made the subject of notice; whenever the disease shows itself in localities which, though not containing marshes properly speaking, are in that condition, we may be sure to find the soil presenting these peculiarities, which elsewhere are associated with the advent of fever, and act injuriously without the necessary coexistence of a notable amount of moisture. Here, consequently, the latter cannot be held up as the efficient morbific agent. It may be added that the instances mentioned by Dr. Fordyce, relating to a soil of *pure sand*, in which water was found in digging less than a foot deep, and occasioned a great moisture in the air, whence arose numbers of fevers; in which the soil while giving passage to the excess of moisture adverted to remained perfectly dry, and in which there did not exist the least portion of organic matter susceptible of decomposition, may be regarded as of a very questionable character, none of them being mentioned on the authority of any responsible writer, and some conflicting in the most positive manner with well-ascertained facts.[2]

[1] American Med. Museum, i. 77.

[2] To Mr. Blodget, to whose Report on the Condition of the Summer of 1853, refer-

A high dew-point not sufficient to account for the occurrence of fever.
—For more reasons than one, objection may be made to much

ence has already been made, we are indebted for the following statements relative to the hygrometrical state of the atmosphere.

The heats of June, in the summer of 1853, were remarkably dry. The fraction of saturation was at a mean of about 50 in the north-east, and but 40 to 45 in the interior; and in Texas, during the hot days, from the 14th to the 17th, though much higher at Pensacola and the extreme south, where the heat was not so great from the 20th to the 23d; the rate was about the same in the districts of excessive heat. On the 29th and 30th, the percentage was but 35 to 40 in the narrow district through Tennessee, Kentucky, and Virginia, which marked 100 as the maximum of temperature.

The first two days of July were a continuation of the condition of the last of June. The remainder of the month was not unusual in its hygrometric character generally, though at New Orleans the evidences of high saturation are given in the profuse and constant rains of the middle of the day, preceded by a hot and oppressive morning. The great heat of August was most remarkable in its hygrometric condition, also, and universally attended with a high fraction of saturation; at Washington, it was 50 to 60; and at New York, where, whatever may have been the sickness and mortality, they did not arise from malarial fevers, and where no yellow fever occurred, the fraction of saturation was near 70 per cent. at 2 P. M., and almost at saturation morning and evening.

Rains in August were excessive.

The temperature of evaporation at New York in August was from 80 to 84, being higher than the maximum temperature of evaporation at New Orleans at any time in 1852, by two degrees; at the latter place, it reached 82 but once in that year; with the exception of New Orleans and New York, at this limited period, the heats of the summer, though extreme, have been attended with a low humidity.

In June, the amount of rain was much less than usual generally; in July, it was particularly large at Philadelphia, and southward to Florida, where it was 11°.5 inches. In Alabama, and at New Orleans, the amount was nearly as great, and in Iowa and Wisconsin, it was again large—from 6 to 8 inches. In some places, there were severe droughts, as in eastern Ohio, western Pennsylvania, and New York. In August, the rains were excessive from the lower part of New Hampshire to northern New Jersey; at Bloomfield, New Jersey, and New York, the amount falling was 12 inches. From Baltimore to Savannah, also, the amount was large, being from 5 to 6.5 inches, and about 1½ inches more than usual, the amount being about 3.5 inches. The last days of July, and the first days of August, gave an excessive precipitation in eastern Pennsylvania and New York, New Jersey, etc. These flooding rains, which gave in some instances 4 to 8 inches in depth of water in a single storm, of a few hours, attended very warm weather, and immediately preceded the heats of the 12th to the 14th of August. These were also followed by profuse rains, and the whole period from the 25th of July to the 15th of August seemed a substitution of a tropical climate for the usually elastic one, in the space of country referred to.—*New York Journal of Medicine*, November, 1853.

Now it must be remarked that the hygrometrical conditions here referred to, extended over a large surface of country, and that while some localities suffered severely from malarial fevers, of various grades and types, others similarly circumstanced in point of humidity, evaporation, and rain, escaped unscathed. To this agent, therefore, we cannot look for the efficient cause of the disease, even when it is combined with high atmospheric heat.

of the theory set forth by our countryman, Dr. C. A. Lee, who, without discountenancing or disproving the existence of an aerial, intangible poison, looks for the main and most efficient cause of fever not, as some have done, in an excess of visible humidity alone, but principally in a high dew-point, however produced (60 degrees at least), which, as he thinks, acts injuriously in some measure, by checking the elimination of that poison from the system, and thereby giving it efficiency, but chiefly (and, as we may presume, exclusively in cases in which the poison does not exist, supposing the occurrence possible, which Dr. L. does not say), by interrupting, to a greater or less extent, the healthy function of the lungs and skin, preventing a perfect decarbonization of, and a sufficient supply of oxygen to, the blood—carrying off the vitreous electricity which acts as a vital stimulus, and, as a consequence, increasing the secretion of bile.[1] Still less disposed must we be to give our sanction to the views suggested, and so ingeniously supported by Mr. T. Hopkins, of Manchester, who seems to ignore malaria altogether, and attempts to identify it and a high dew-point, accounting for the production of fevers solely by the check thereby given to evaporation from the surface of the body.[2]

That a high dew-point has a tendency to produce injurious effects on the system; that it is often found to exist in unhealthy localities, or during pestilential times; and that it must assist somewhat in the development of autumnal and periodic fevers, are facts which no one will question. But that it plays the all-important part in the causation of those diseases attributed to it by Dr. Lee, and particularly by Mr. Hopkins, is far from being demonstrated. Were the belief correct, we should expect to find that a high dew-point—reaching to at least 60 degrees—is always attended with the occurrence of malarial fever in one or other of its various forms; that countries or seasons in which the dew-point is high, are necessarily insalubrious; that this condition is proportioned to the elevation of the dew-point; and that, on the contrary, localities in which the degree is low, are as necessarily healthy. Now let us inquire how far facts will bear us out on these points. We have seen that fevers, even of a

[1] Forry, Climate of the U. S. 112, 113. In a note, appended by Dr. Lee to the article Intermittent Fever, of Copland's Medical Dictionary, he is quite orthodox on the subject of Malaria, the existence and morbific agency of which he fully admits. Not one word is there said about the dew-point.

[2] Lond. and Edinb. Phil. Mag. 3d S. No. 86, Feb. 1839; An. d'Hygiène, 25.

severe and malignant character, have not unfrequently broken out, and spread extensively, in places where but little visible humidity existed; where the soil was arid, dry, and cracked, and everything upon it was parched in consequence of long-continued and severe drought; where the absence of rain was not compensated by fogs and dews, and where, as a natural result, the dew-point was found, or might be inferred, not to have reached very high. We have seen, on the other hand, that the same or other places, have remained exempt from those diseases, under hygrometrical conditions of an opposite character, combined with a degree of heat well calculated to promote an excess of atmospheric moisture, visible and invisible; and when, from these circumstances, the dew-point, if not positively ascertained to have been high, may, with perfect propriety be supposed, in the absence of direct observation, to have risen considerably. It may be remarked, also, that in places subject to occasional visitations of autumnal fever of various grades of intensity, sickly seasons are not necessarily characterized by a higher dew-point than the corresponding periods of other years in which the disease does not make its appearance.

In Demerara, the height of the dew-point in ordinary seasons differs but slightly from that to which it reached during the prevalence of the fever of 1837–1845. In 1843, the mean degree was 73.8, varying from 72. (Feb.) to 74.9 (July, Aug., Nov.) In 1844, the average height was 75.1, with a variation of from 73.3 (Dec.) to 76.0 (May). In 1845, eight months give us a mean of 74.6—from 73.0 (Jan.) to 76.0 (April), while, in 1846, the year after the cessation of the disease, the mean dew-point was 74.4, with a variation of from 70.8 (Feb.) to 76.2 (May and June).[1]

The following table of the dew-points during the months of June, July, August, September, October, and November, from 1845 to 1851, and July, August, and September, 1852 inclusive, at Charleston, S. C., will show to those who are aware that the yellow fever prevailed there during only two of those years (1849 and 1852), that the difference in regard to the saturation of the atmosphere during sickly and healthful seasons is but trifling, and cannot alone account for the production or absence of the disease; and, indeed, that the results obtained are very different from those that might have been expected did a high dew-point and fever stand in the relation of cause and effect.[2]

[1] Blair, *op. cit.* 120.

[2] Hume, Charleston Journal, v. 10; viii. 67.

	June.	July.	Aug.	Sept.	Oct.	Nov.
1845	69.46	72.77	71.96	66.00	54.61	41.00
1846	70.83	72.82	75.64	70.66	59.22	50.46
1847	71.74	73.39	74.23	68.20	58.67	53.53
1848	70.26	73.00	72.09	66.26	58.74	44.80
1849	73.46	70.77	72.71	66.00	60.93	53.13
1850	67.86	75.03	74.93	69.70	55.00	50.86
1851	70.73	74.38	74.67	65.13	57.41	48.23
1852		73.20	68.77	68.		

Thus the average dew-point in June and October of the sickly year of 1849, was higher than in the corresponding months of any year of the series; but when we examine the results obtained in the other months, matters are found to take a different turn. For while in July, 1849, the average was 70.77, and in 1852, 73.20, we have a higher point than in the former in each of the other years of the series, and a higher one than in 1852, in the same month of 1847, 1850, and 1851. The average in August, 1846, 1847, 1850, and 1851, was higher than in 1849, and higher than in 1852 in every year of the series. September in 1846, 1847, 1848, and 1850, gave a higher point than in 1849, while the amount in 1852 was exceeded by that in 1846, 1847, and 1850.

In New Orleans, where the average annual dew-point, calculated on a basis of eight years, was found to be 63.56 (observed at 12 o'clock), June being 73.95; July, 75.42; August, 75.59; September, 73.63; October, 62.73; November, 54.27; the year 1849, during which both the yellow fever and cholera prevailed extensively, presents an average for the twelve months of 63.71: June, 74.43; July, 75.53; August, 76.75; September, 73.78; October, 61.16; November, 57.71.[1] Indeed, the physicians of Louisiana have had frequent opportunities to observe the dew-point for months together only a few degrees below the atmospheric temperature, without, however, suffering from any extraordinary prevalence of intermittents.[2]

The above statements show, that the mean dew-point of the summer in Charleston and New Orleans, is several degrees above that of the autumnal season, when fevers are more apt to be prevalent. In the latter city, the excess amounted to near nine degrees. In Charleston, the difference between the mean dew-point

[1] Southern Med. Repts. i. 100; ii. 148.

[2] Carpenter, N. O. J. iii. 428.

of the three summer months of the seven years specified, and that of the three following months, reached above fourteen degrees. Dr. Gardner, in his essay on the dew-point, states the excess, in the United States generally, to be upwards of fifteen degrees. It is evident, also, from facts mentioned already, and from many others that might, if necessary, be added, that in all parts of this and other countries—in localities frequently or occasionally visited by yellow or other forms of malarial, fever—a dew-point of sixty or more degrees is often experienced months together without ill health, although the temperature be such as to promote the production of disease. On the other hand, as Dr. Gardner properly remarks, the fens of Lincolnshire, Walcheren, the marshes of Holland, are pestiferous with a dew-point of less than fifty degrees.[1]

From these circumstances, viewed in connection with the fact so often referred to, that fevers occur in ships and circumscribed localities, while other ships and localities in the immediate vicinity, and placed under like meteorological influences, remain exempt; that the alleged effects are not found to be produced by a saturated atmosphere in the higher latitudes; that seamen at sea are exposed with perfect impunity to an atmosphere saturated with moisture, and are attacked only when they land, indicate plainly the impropriety of attaching too much weight to the hypothesis in question, and must debar us from the possibility of connecting, as efficient cause and effect, a high dew-point and malarial fevers of various grades. The impropriety of the belief is farther proved by circumstances which Dr. Gardner appears to have established, *i. e.* that the detention of the insensible vapour from the lungs or the skin, by a high point, is much less considerable than it is represented by those who have urged the theory; that the dew-point in insalubrious places is often much lower than is thought, and that a high point does not arrest the separation of carbonic acid and the other constituents of sweat. Let us examine the effects produced as we may, and concede to a high dew-point what extent of agency soever we may think to be warranted by the nature of the facts observed, as well as by the well-known influence of atmospheric moisture, we arrive always at the conclusion that something more is necessary to enable us to account for the development of fever; that, by itself, a high dew-point will not occasion the disease; that, therefore, its

[1] American Journal, ii. N. S. 107.

agency is of a secondary character, and consists, perhaps, as Dr. Lee supposes, in checking the elimination of the febrific poison, but more surely in predisposing the system to the action of the true efficient cause, by hindering a natural escape from the blood of those materials which constitute the liability to the disease, or maintaining them in a condition favourable to the morbid change; as, also, like visible humidity, in promoting, when aided by other influences, those reactions, between the remains of organic and inorganic matters on which the evolution of the morbific agent depends.

Heat and humidity combined not the efficient cause of fever.—From the foregoing facts and considerations, we may conclude that neither heat nor moisture, when acting separately, can be productive of autumnal and kindred fevers. Equally objectionable is the belief that such diseases arise from the combined influence of those two agencies. In most of the instances mentioned, humidity, when it existed, was associated with atmospheric heat, and yet the combination failed to prove injurious. Dr. Drake remarks, in corroboration, that autumnal fever seldom appears on board of vessels which cruise in the Gulf of Mexico, although the air, at the temperature of 80°, is nearly saturated with vapour; that the inhabitants of Key West, who breathe a similar atmosphere, are much less afflicted with the fever than those on the Peninsula of Florida, several degrees farther north; that the pine woods around the Gulf of Mexico, at the distance of only two or three miles from the estuaries of the rivers, are places of retreat from fever, although there is a sea and land breeze which tends to equalize the humid atmosphere; that the inhabitants of the Balize suffer less from the fever than those along the rivers of the interior of Louisiana, two or three degrees farther north, notwithstanding they are immersed in an atmosphere of great heat and vapour; that, at our different salt works, the operatives spend their lives in a hot atmosphere, saturated with vapour, and yet, on the whole, are more exempt from fever than the surrounding population; and, lastly, that in some of our manufacturing establishments, the in-door artisans and operatives labour in a heated atmosphere supersaturated with vapour, but remain free from autumnal fever. To this it may be added, that in the plains of Meta, situate on the east side of the Cordilleras, fever does not prevail extensively, while in the valley of the Magnalena, on the west of those mountains, it is of almost constant

occurrence; yet the state of the barometer, as well as the hygrometrical variations, are the same in both places.[1]

These various facts, showing the innocuousness of the combination in question, lead to the inference that, when the prevalence of fever happens to be associated with the existence of heat and moisture combined, the cause of the disease is not to be sought in the latter, inasmuch, particularly, as we find that febrile complaints occur often, and spread widely and fatally, without any such combination. Humidity, therefore, if instrumental in producing fever—and no one will deny the fact—must, as already stated, act in a secondary capacity; and, instead of being regarded as the efficient cause when alone or combined with heat, must be viewed as merely aiding in the elimination of the latter, and predisposing the system to its morbid effects.

Vicissitudes of temperature will not account for the occurrence of periodic fever.—Neither can we admit the propriety of referring the efficient cause of autumnal and kindred fevers to the difference of temperature between day and night, or to mere atmospheric vicissitudes—the succession of cool or cold nights to hot days; nor the sudden exposure of the body, at any period of the twenty-four hours, to a low degree of temperature after it has been placed for a greater or less extent of time under the influence of a high degree. The agency of such vicissitudes were noticed as early as the days of Hippocrates. Hoffman sets them down as the general remote cause of epidemic fevers, and after him, several writers, among whom Broussais[2] and Dr. Dundas[3] figure conspicuously, have laid much stress on their efficiency, and displayed considerable ingenuity, in endeavouring to explain the manner in which the morbid effects are brought about. But after carefully examining what the advocates of the opinion have adduced in its support, it appears to me that were atmospheric vicissitudes the efficient agent in the production of periodic and other forms of malarial fever, we might expect to find these diseases prevailing principally in seasons in which the number of dew or cold nights following on hot days is greatest. We should be justified also in expecting malarial fevers

[1] Boussingault, An. de Chimie, lvii. 153–4.

[2] Commentaires des Propositions, No. 227, pp. 672–3.

[3] Sketches of Brazil; including New Views on Tropical and European Fevers. London, 1852, 205.

to occur not once or occasionally, but frequently, if not universally, whenever the supposed cause manifests itself; or, rather, it ought to be found that every time such fevers prevail—sporadically or epidemically—the difference of temperature between day and night is greater than in healthy seasons. We should besides expect to find them appearing, not sporadically only, but in an epidemic form, as well in clean, well-paved, and well-aired cities, where atmospherical vicissitudes are as apt to be felt as elsewhere, as on the borders of creeks, rivers, and lakes, and in meadow lands, in level plains or marshy localities; and whenever a man whose body has been overheated is suddenly exposed to a cold atmosphere, or plunged into a cold bath, he ought to be regarded as no less liable to suffer from an intermittent, a remittent, or a yellow fever, of the most legitimate kind, than from a pleurisy, a catarrh, or any other kindred disease. But so far from this being the case, experience shows that fevers occur and prevail extensively in situations where, and at periods when, such vicissitudes are not felt at all, or are so to too inconsiderable an extent to be productive of the baneful effects ascribed to them; and, on the other hand, that those diseases are either seldom felt or completely unknown in localities where or seasons when, sudden changes of temperature, or the contrast between night and day, are as common and noted as, if not more so than, in places, and at times remarkable for insalubrity. Surely a morbid agent which, if it really exercises any influence in the production of autumnal fevers, does so only in localities of a special kind, where, let it be remembered, those fevers often appear and even abound without its aid; an agent which habitually fails to produce those same fevers in localities of a different kind; which produces the effect ascribed to it only in a certain season of the year, however manifestly it may show itself at other periods, and whose known ordinary products everywhere and at all times, are diseases very different in every respect from those in question; such an agent, I say, cannot lay a just claim to be held in the light of the efficient or necessary cause of the latter.

The climate of Newfoundland is marked by sudden and frequent vicissitudes of atmosphere; notwithstanding which, however, fevers are scarcely known there, even among those most exposed.[1] The whole coast of this continent, from one extremity to the

[1] Tullock, Sickness, &c. of British Army in British America, 35.

other, is proverbial for the frequeney, suddenness, and extent of sueh ehanges; and yet, while some loealities are annually the seat of the pyrexial complaints under consideration, others, equally subject to the former, remain untouehed by the disease. Nay, vicissitudes, if really the efficient cause of malarial fevers, appear to be whimsieal in their operations. See, for example, how they eomport themselves in Charleston. The autumnal seasons of 1849 and 1852 were decidedly malarial. During the months of July, August, and September of the first, the number of nights of dew, with high temperature during the day, amounted to 51, and far exeeeded that of 1845–8 and 1851, and by 8 that of 1850, in neither of whieh years did the yellow fever prevail. So far, therefore, there seems to be some connection as cause and effeet between eold nights with hot days and fever. But when we eome to inquire into the results obtained in 1852—whieh was a decidedly malarial year, as evideneed by the occurrence of both the yellow fever and dengue, we find that the number of dew-nights amounted only to 39, or 4 less than in 1850, when there was no malarial developments, and the range of the temperature was greater. For this faet, we are indebted to Dr. Hume,[1] already referred to. The range in 1849, during July, August, and September, amounted to 1,524 degrees (the minimum or night temperature being 63.92, and the maximum or dry temperature being 79.16), and exceeded by far that of 1845–6–7–8–50–51. But the range in 1852 was only 1,284 (minimum 66.25, maximum 78.91), being only 26 degrees higher than the range of 1851, and less by .82 than that of 1850, when the minimum was 68.00, and the maximum 81.58. In a word, the resemblance of 1850 and 1851 with 1852, is too striking in this and other respeets, to make us attaeh much importance to this point.[2]

It may not be improper to remark, 1st, that persons who take the fever are seldom sensible of having felt the effects of atmospherie vieissitudes, and often have been plaeed, purposely or aeeidentally, beyond the possibility of experieneing these—supposing them to oeeur. 2d. That the cold, ineident to sueh vieissitudes, acts on organs different from those eoncerned in the development of periodic or malarial fevers. Produeing its first impress on the skin, it affeets most frequently those parts whieh aet as supplements to that eliminating tissue—the lungs, and the urinary passages. Other

[1] Charleston Journal, viii. 67.

[2] Hume, *op. cit.* 58.

parts, of course—even the abdominal viscera—are sometimes implicated, but the symptoms which then occur bear no resemblance to those of autumnal fevers, unless other causes have co-operated. That the cold felt under the circumstances is instrumental in exciting or multiplying attacks of malarial fevers, is no doubt true; but it does in regard to these diseases what it effects in regard to many other complaints; and it would be wrong, from its agency in those cases, to argue that it is the efficient cause of the disease; for, alone, it cannot give rise to anything of the kind. More rational is it to view it as having simply aroused from its slumbers a morbific influence floating in and impressing the system in a slow and invisible manner, and requiring, for its outward manifestation, the disturbing influence of some other agency. The same result attends the action of atmospheric vicissitudes in regard to diseases of a specific nature, and which no one would have the hardihood to ascribe solely to such a cause. What the latter does as respects malarial fevers, it does, for example, in regard to mercurial ptyalism in persons whose systems have been impregnated with mercury; but in whom the affection of the mouth has not yet been developed. In such instances, exposure to atmospheric vicissitudes—to cold, however applied, especially when the system has been previously heated—will hasten and increase the elimination of the poison, and salivation will be established. The same remarks apply equally well to hydrophobia, which has not unfrequently been developed through the agency of the cause in question. Nothing is more dangerous than the action of atmospheric vicissitudes, or cold, on individuals bitten by the lanceolated trigonocephalus of the Antilles. The same injurious effect is said to be experienced by individuals who have been exposed to the action of the mancinella and the Rhus toxicodendron.[1]

In all these instances of change from heat to cold, the latter has acted simply as an exciting cause, and must be ranked with, for it acts much in the same way as, several other agencies—intemperance in eating or drinking, venereal excesses, exciting and depressing passions of the mind, &c.—which no one in his senses can feel disposed to regard as capable of producing periodic or malarial fevers, any more than of producing ptyalism or hydrophobia.

[1] Boudin, Géog. Méd. 67.

The attack is sometimes too sudden, and follows on too transient an exposure to infected regions, without appreciable atmospheric vicissitudes, to be the effect of these or any other kindred cause.—It is not less true that the attack comes on often too suddenly, is too palpably connected with a transient exposure to an unhealthy spot; and appears under circumstances which too evidently shut out the idea that atmospheric vicissitudes have had an agency in its production, to justify us in viewing these in the light of the efficient cause of the disease. Who has not seen or read of persons being seized with intermittent, remittent, or yellow fever, after a single and momentary exposure, not at night only, but sometimes during the day, to the atmosphere of infected localities, who, before such exposure, had borne, with perfect impunity, as thousands of other individuals were continuing to do, the thermometrical vicissitudes of neighbouring localities. The facts on the subject mentioned by Humboldt, respecting persons taking the yellow fever in consequence of *passing through* Vera Cruz in a litter, with a view to embark, are familiar to most readers,[1] and others of similar import may be easily gathered from the annals of periodic fevers. I could, if necessary, relate several instances of individuals who have been seized with yellow fever after remaining but an hour or two in the city of Havana. The history of all our epidemics furnish us with examples of the sort. In 1798, Dr. Cooper, who was residing at Bush Hill, imprudently visited the infected district, and was a short time after attacked. During the fever of 1820, in this city, a young man, residing in a healthy district, jumped over the barricades, walked through the deserted streets for a few moments, and paid the penalty of his imprudence. "If my own frequent observations," says Macculloch, "show that fever may be induced within half an hour after exposure to malaria, and that a single inspiration, in the space of a very few seconds, is amply sufficient for the purpose, this is also an opinion most decidedly stated by many French and Italian physicians, whose experience and acuteness will not be questioned. It is equally the opinion of others elsewhere, not physicians, and therefore without the bias which might be suspected in such cases; of military, and chiefly of naval men, whose observations have been founded on the momentary and transitory effects of a breeze of wind, and especially of a land-wind

[1] Nouvelle Espagne, 4to. ed. 773-4.

blowing off to sea. In France and Italy, to confirm this, instances are known and recorded, of labourers dying instantaneously from merely sitting or lying down on the ground, and of others who, from looking into a ditch or drain, have been struck dead by that poison, which, of course in a minor degree, has merely produced a fever."[1] But as these statements may be objected to on the score of the tendency to exaggeration with which Macculloch has, perhaps not unjustly, so often been charged, it may not be improper to remark that the fact of infection after transient exposure, derives ample support from the asseverations of other writers whose testimony cannot be impugned. Lancisi relates that of a party of thirty gentlemen and ladies of rank in Rome, who made an excursion towards the mouth of the Tiber, the wind suddenly shifted, and blew from the south over the marshes, when twenty-nine were soon seized with a tertian fever.[2] "The marines," says Lind, "who were three times a week exercised early in the morning on South Sea Beach (in 1765), from the effect of the stagnant water of an adjoining morass, suffered much. Half a dozen of them at a time were frequently taken ill in their ranks, when under arms; some were seized with such a giddiness in the head that they could scarcely stand; others fell down speechless, and, upon recovering their senses, complained of a violent headache. When such patients were received into the hospital, some few had a regular ague, but far the greater number laboured under a remitting fever, in which sometimes, indeed, there was no perceptible remission for several days."[3] Ferguson says, of Monk's Hill: "It was the duty of the white troops, in both these forts, to take the guard and duties of the dock-yard amongst the marshes below, and so pestiferous was their atmosphere that it often occurred to a well-seasoned soldier, mounting the night-guard in perfect health, to be seized with furious delirium while standing sentry, and, when carried back to his barracks on Monk Hill, to expire in all the horrors of the black vomit, within less than thirty hours of the first attack."[4] The same writer states that, during the advance of the British army from Oporto in 1809, the troops took up their bivouac in the dried-

[1] Essay on Remittent Fever, &c. i. 16.

[2] De Nox. Palud. Effl. lib. ii. cap. 3.

[3] Dis. of Hot. Clim. 24, 5. See, also, 32, 89; 159–160; 240–242. *Ib.* on Seamen, 81.

[4] Marsh Poison, in vol. of Recollections, 194–5.

up beds of the mountain streams. These localities proved highly unhealthy, and several of the men were seized with violent remittent fever before they could move from the bivouac the following morning.[1] Dr. Celle, in his *Hygiène des Pays Chauds* (79), relates the following instance of the sudden influence of miasma: "In 1844, eight soldiers, an officer, and a custom-house agent, were sent from San Blas to the environs to surprise a convoy of silver which had been reported as about being embarked by contraband. They concealed themselves in a thicket, close to a marsh. An hour after, they were all seized with vertigo, nausea, vomiting, and an excessively violent chill; in such a way, indeed, that the convoy passed without one of the men being able to rise from the ground to seize it." The same writer states that similar occurrences are noticed among the soldiers and custom-house officers stationed at the entrance of the port of Mazatlan. The station is situate between the sea and a marsh. The men are on duty during twenty-four hours only, yet, at the end of this short time, it generally happens in the rainy season that they all return with fever or dysentery (80). "It is a curious thing," says an official document, "to see six men sent ashore in perfect health, return on board ten minutes after, all six labouring under fever."[2] Nepple relates the history of eight men who, during the hot weather of August, 1825, were employed in thrashing in a barn situate in the immediate vicinity of a source of infection near the town of Montluel. They were all seized the same day with intermittent fever of greater or less violence.[3]

Before the English raised the blockade (of Batavia) parties of men and officers were sent on shore at Edam, to blow up and destroy the works and buildings on the island, which operations detained them about half the night there. Most of them were shortly afterwards attacked with the fever.[4] Mr. Lander, surgeon to Lord Rothe's regiment of horse, communicated the following information to Sir John Pringle: Most of the men were first taken ill upon their return from forage; for, the regiment being cantoned upon the right and left of St. Michael's Gestel (their principal quarters), close upon the inundations, and many of the quarters being above two leagues from Bois-le-duc, where the magazines

[1] *Ibid.*

[2] Admiralty Reports.

[3] Fièvres Intermittentes, 142. See, for other cases of similar kind, Second Rep. of Lond. Commiss. for 1848, 96; Bryson, 205; Johnson on Trop. Climates, 127.

[4] Johnson on Tropical Climates, 134.

were kept, the men were obliged to set out about four in the morning, in order to get back before the greatest heat of the day. At this early hour, the meadows and marshes on each side of the road were covered with a thiek fog of an offensive smell, which Mr. L. eonsidered as the chief cause of the sickness. For, although the party generally returned before noon, several among them were already in a fever, and some aetually delirious; nay, a few on their way home were so suddenly taken with a frenzy, as to throw themselves from their trusses into the water, imagining that they were to swim to their quarters.[1]

Mr. Grainger, in his testimony before "the London Commissioners," quotes a letter from a medical practitioner: "I was passing the drain-grating at the corner of Union and Bond Street, when I pereeived a most faint and disagreeable smell arising from it. Being immediately attacked with nausea and an indescribable sensation of illness, I at once returned home, and drank half a wineglassful of brandy. After a short time the indisposition appeared to pass away, but the peculiar smell of the drain still remained in my nostrils. I had to visit a patient near Manchester Square, that evening, and whilst in the house, I felt so ill that I immediately returned home and went to bed. At that time my head ached violently. I had nausea, pain in my back, and an unpleasant taste in my mouth.[2]"

All these cases, and others equally striking, which it would be easy to adduce, show, beyond the possibility of denial, that periodie or autumnal fevers, of all grades and forms—from the most trifling to the most malignant—are often produced almost suddenly by the shortest possible exposure to sickly localities; and it is certainly difficult to understand how atmospheric vicissitudes can alone be looked upon as the chief agent in the production of such diversified effeets; seeing espeeially that the morbid changes to which they usually give rise differ materially from those under present eonsideration. Add to this, that atmospheric vicissitudes are not greater, and cannot be more deleterious near to, or to the windward of a morass, than at a short distanee from, or to the leeward of it; along the margin of a lake or river, than on its surfaee a few hundred rods off; in a sickly port than at a short distanee outside; and that, if they produce the fever in the former, there is no reason why they should not do so equally well in the latter.

[1] Pringle, 177–8.

[2] Rept. 1848, 96.

Fever not the effect of a particular electrical state of atmosphere.—Not more satisfactory is the hypothesis that the efficient and necessary cause of autumnal fever must be sought in some particular electrical or magnetic state of the atmosphere. On this subject we know too little to form even a plausible conjecture; and I hazard nothing in affirming that the various theories of Giannini, Murray, Craig, Shecut, Rumph, Pallas, and others, have failed to satisfy medical inquirers of sound judgment. The sphere of prevalence of the disease is often circumscribed within such narrow and well-defined boundaries, that it is difficult to perceive the propriety of regarding it as due to any modification in an agent of the general and wide-spread character, necessarily possessed by the one under consideration. This modification, whatever it may be, whether it consist in an excess or deficiency of the fluid, extends far and wide, and cannot fail to exercise its influence, as well at a distance from, as within the precincts of, the sickly district; and were the theory correct, we should not hear of, and every day see, instances of fever being arrested by a street, a wall, or the like; or extending its ravages over a very limited surface of ground, in a single ship, or side of a ship, on one side of a street, &c. The same modification in the electricity of the atmosphere must have existed beyond those narrow limits; and if it were the sole cause of the fever on one side of the barrier, it would in all probability have produced a similar effect on the other. Some other agent, then, must have been at work in the former, which did not exist in the latter. It matters not whether the sickly locality covers a small area of ground, or extends over a wide surface, the necessity of that peculiar agent must be admitted; and while it would be improper to deny that a modified state of atmospheric or terrestrial electricity exercises an influenee in the formation of the supposed agent, and, in addition, predisposes the system to be morbidly affected by it, we must necessarily absolve it from the charge of acting as the sole efficient cause of the disease.

The preceding facts lead to the belief in a poisonous agent floating in the atmosphere.—All the above hypotheses failing to account for the effects in question, we are naturally led to the admission that they are produced by the morbifie influence of some special agent; and when we take into consideration all the circumstances attending the appearance of febrile diseases, the circumscribed sphere of their

prevalence, the suddenness of their attack, the character of their phenomena, &c., we may safely say that there is nothing left but to attribute them to the action of some poison dissolved or suspended in, and contaminating the air of the infected locality; which poison, while doubtless requiring for its development and dissemination a certain degree of heat and terrestrial and atmospheric moisture, a certain amount of nightly condensation after evaporation, and the presence of fermenting or decomposing materials, cannot be produced by either of these agencies alone; and, though undetected by the chemist, betrays its presence by producing on those exposed to its influence the peculiar morbid changes characterizing fever.

The innocuousness of some marshes, and of localities similar to those that are sickly, no proof of the non-existence or non-agency of malaria.—That marshes and swampy surfaces—using these terms in their more enlarged sense—collections of organic substances in a state of decomposition, &c., do not occasion the effect in question *always* and *everywhere;* that ships at sea and in port, though often superabundantly filled with such materials of decomposition, are not invariably the seat of febrile developments in a sporadic, and especially in an epidemic form, all must know who have paid attention to the subject. During particular seasons, in places presenting some of the peculiar characteristics specified, cases of fever abound. During others, and sometimes during a succession of seasons, the disease is scarcely, if at all seen. In the quebradas of Peru, fevers are often general, and assume the garb of wide-spreading and malignant epidemics. This continues a year or two, during which the mortality becomes very considerable. At the end of that time a favourable change takes place, and the country once more becomes healthy.[1]

In a medical report of the House of Recovery of Dublin for 1829, Dr. O'Brien says: "Intermittent fever has been a rare disease in Dublin, and, we may add, in Ireland. During a period of twenty years, through which the author's experience extends, a few incidental cases only of the disease occurred, from time to time, in his hospital and dispensary practice, and those few were persons who brought the disease from England. A singular revolution has taken place in Dublin for the last two years with respect to intermittent and continued fevers; the latter disease, which, for a cen-

[1] Ulloa, Mem. Philos. i. 245.

tury, had been the constant scourge of this city, has considerably declined, and intermittent fever has sprung up, and occupied its place. For the last four months, however, up to February, 1830, intermittent fever has again disappeared, and I am happy to say, without a corresponding increase of continued fever."[1]

I shall not stop to show that the same alternation of healthy and sickly periods in localities where, from the nature of the soil, and some of their external conditions, fever might at first sight be expected to arise annually, has been noticed in this country and elsewhere. The task would offer no difficulty, for facts to the point could be abundantly supplied by the *Transactions* of our State Medical Society,[2] and a variety of works of easy access. But the subject has already been alluded to, and may be reverted to again under a different head, and must, besides, be familiar to all well-informed and observant physicians.

It not unfrequently happens that in localities where, from the character of the soil, or the nature and condition of the substance by which the surface is covered, as well as from the heat and moisture of the climate, febrile complaints might be expected to occur, they are, nevertheless, seldom or never encountered. We have already seen that many parts of Scotland and Ireland that are occupied by large tracts of peat moss are completely free from these fevers, and that the disease is never seen among the inhabitants of the country bordering on the Dismal Swamp between Virginia and North Carolina. The numerous and extensive marshes of Sweden, somewhat beyond the sixtieth degree of latitude, of Norway, of Russia—near St. Petersburg, for example—of Laponia, &c., have, it is said, little influence on the mortality or salubrity of those regions; intermittents and remittents not being endemic there;[3] and every one knows that, whatever be the condition of the towns and cities of these regions, or of Northern Europe generally, yellow fever, properly so called, has never, except perhaps at Copenhagen—and even this is open to doubt—made its appearance there.

Intermittents are almost unknown on the banks of the Rhine from its source to Chur. Making their appearance near this place, they

[1] Dublin Med. Trans. N. S. i. 330–1.

[2] See Trans. of Med. Soc. of the State of Pennsylvania, ii. 51, 93.

[3] Monfalcon, 26; Acerbi Viaggio al Capo Norte; Brocchi, *op. cit.* 277; Brit. and For. Med.-Chir. Rev. x. 367; Boudin, An. d'Hygiène, xxxiii. 116; Bang. Prax. Med. 36.

continue to show themselves as far as Maïenfeld, where they again disappear, to break out anew, and prevail from Strasburg to Bingen. At this spot they once more are lost sight of, and are not seen before reaching Cologne, and especially the delta of the above-mentioned river.[1]

On the summit of the group of Calabrian mountains, denominated La Sila, the larger portion of which, formerly covered with forests, is at present under culture, are to be found extensive marshy localities. One of these, situate in the valley of the Crocifisso, between Cecio and Camiliati, is very large. From these marshes arise thick fogs, which float on the surface to the height of many feet, and continue to do so till dissipated by the action of the sun; yet the peasants sleep with perfect impunity along the margin of these marshes during summer and autumn, and the proprietors reside there the whole of that time in the Hotel or Casino of Cecio.[2]

It will be found that, in cold climates, periodic or malarial fevers, generally considered, diminish in frequency in proportion to the elevation of the latitude, but that in so doing they conform less to the direction of the parallels than to that of the isothermal lines. M. Boudin,[3] who has called attention particularly to this circumstance, remarks that, though but little prevalent at St. Petersburg, which, nevertheless, is surrounded with marshes and situate under the 59° of north latitude, they cease to exist in Asia towards the 57°, whilst in Sweden, they show themselves as far as the 63° of the same latitude, and even reach, somewhat farther in a westerly direction, the Shetland Islands, and even Iceland. From this, it results that the northern limit of intermittent fever is in some measure represented by the isothermal line determined by an annual temperature of 5° centigrade, with a mean of 0 in winter, and 10° in summer; and that this line lowers in Central Asia and in North America below the 50° of north latitude; whilst between those two continents and on the Atlantic Ocean it rises as high as the 67° of the same latitude.

The marshy grounds on the south-west coast of Ceylon, between Negombo and Galle, do not seem to exercise much influence in the production of fever. Again, the island of Mauritius, in the Eastern hemisphere, resembles Jamaica in its physical characters. The

[1] Boudin, Fièvres Intermittentes, 58.
[2] Brocchi Stratto Fiscio del Suollo di Roma, &c. 278.
[3] Geographie Médicale, 16.

temperature is the same; it lies nearly in the same latitude, but to the south of the line; morasses and marshy land occur as frequently in the one as in the other, and yet periodic fevers are rare in the Mauritius, and the mortality small from this cause; while, in Jamaica, the reverse is the case, remittent fever being a common and fatal disease.[1]

In many parts of New England, as far as the St. Lawrence, New Brunswick, Nova Scotia, and Lower Canada, Forts Moultrie, and Munroe, Hancock Barracks, West Point, etc., fever is seldom, if ever noticed, though, judging from the external appearances and position of those localities, some of which are at the outlet of rivers, and exposed to that combination of mud and marsh, regarded as its prolific source in other parts, there is nothing by which we can account for the exemption.[2] Dr. Drake, who has paid considerable attention to this subject, remarks that the geographical limits of fevers in this country are east of the Appalachian Mountains below the 33° of latitude, beyond which those mountains do not extend. Below that parallel, it has no eastern limit but the Atlantic Ocean. South-west, the Cordilleras of Mexico and the Southern Rocky Mountains, constitute its boundaries. In higher latitudes, it ceases in the great plains of our western desert, long before we reach those mountains. It is almost unknown at the distance of three hundred miles from the western boundary of the States of Missouri and Iowa, above the latitude of 37° N. To the north, it does not prevail as an epidemic beyond the 44° parallel, and ceases to occur even sporadically about the 47°.[3]

Dr. Carpenter states that on the bayous of the Louisiana delta, Lafourche and Terrebonne, for example, the habitable land is limited to narrow strips, of from a few hundred feet to a mile in width, which form the banks of the streams, and follow their windings, and which are surrounded on all sides by swamps and marshes, in some places, and in others, open and exposed; yet, notwithstanding, these regions are more exempt from fevers than almost any other portion of the State.[4] Diseases arising from malaria might be expected to be very prevalent in the valley of the Sacramento, containing, as it does, thousands of acres of land subject to annual overflow, and from which there is no escape for the water, except

[1] Tullock, Sickness and Mortality of British Army, 16, 45. B.

[2] Forry, *op. cit.* 278–9.

[3] *Op. cit.* 704.

[4] Carpenter on Periodicity, N. O. J. iii. 429.

by the slow process of evaporation, under the rays of an intensely hot sun; yet, notwithstanding this, cases of intermittent fever are comparatively rare in this part of the valley. A few mild cases of intermittent fever occurred in the neighbourhood of the city last spring; but they appeared to be confined to low localities which are situate in the immediate vicinity of stagnant water.[1]

Dr. Tilton, in a communication to Dr. Currie, states that the town of Lewes, on Cape Henlopen, in the State of Delaware, is so salubrious, that the inhabitants are the longest-lived in the State, and persons affected with enlarged spleen, and obstructed viscera, from repeated attacks of fevers, are speedily restored to health by a residence there. Yet, the Cape is perfectly surrounded with marshes. The same thing is observed at Bombay Hook, another town of the same State.[2]

But such facts, and numerous instances of vessels, which, though filthy in the extreme, remain free from fever, upon which Dr. Dundas and other opponents of the doctrine of malaria seem to attach so much importance, do not, true as many of them doubtless are, militate against the views here advocated. Neither can much be made out of the fact, often adduced, that the true yellow fever has seldom prevailed farther south than the Amazon River, which divides Brazil from Guiana, though materials for decomposition, terrestrial and atmospheric humidity, and high temperature, exist to as great an extent south as north of that point; that its proper soil is to be found in the West Indies, on a small portion of the coast of South, and a larger one of North America, and of Western and Eastern Africa, embracing an area which extends from the aforesaid river to Charleston in one direction, and from Barbadoes to Tampico in another—to say nothing of its appearing occasionally as low as New York, or even Boston, as well as in some parts of southern Europe; that the disease, indeed, can scarcely be said to appear south of the equatorial line, sparing the Atlantic and Pacific coasts of America, south of the Amazon; and that it seldom or never appears in the East Indies, where, notwithstanding all that Valentin, Ffirth, Pugnet, Lind, Bancroft, Chervin, and many others may have said to the contrary, autumnal fever assumes the character of the bilious remittent, and not of the true typhus icterodes.

[1] Blake, On the Climate and Diseases of California, N. O. J. ix. 510.

[2] Currie; Hist. Account of the Climate and Diseases of the United States, 211, 218.

In reference to the argument against the malarial origin of fever derived from the exemption from the disease in question beyond the limits thus assigned to it, it may be remarked that, admitting the statements on which it is predicated to be correct, the exemption in question could not be used successfully to disprove the necessity of morbid exhalations in the production of the fever; for, if it be true that such exhalations—which must exist south as they do at the equator and north of it—do not give rise to the disease beyond the limits mentioned, the opponents of malaria will have to explain how it happens that the causes to which they assign it, and which they must allow exist in as full force on one side as on the other of the line, do not occasion the effect. If such causes are, from some unexplainable influence, incapable of producing that form of fever south of that line, while they occasion it in the other direction, there is no reason why the same may not be the case with respect to exhalations. But we may go farther, and deny the statements respecting the absence of yellow fever south of the equator; for, though it be true that in South America the disease is not of such frequent occurrence as in the West Indies, and on the coast of America at or north of the line—though in India the fever seldom, if ever, appears in its legitimate garb, there are not wanting facts, of the most unequivocal character, to show that yellow fever has made its appearance, and prevailed extensively and fatally beyond the southern limits mentioned. Of course it will not be denied that such has often been the case on the African coast. The epidemics of Sierra Leone, Ascension, Boa Vista, &c., are there to settle that point. Turn we to the American continent, and we shall find that the fever has prevailed at Guayaquil;[1] that it has shown itself at Callao;[2] and that it has not spared Montevideo.[3] Nor should it be forgotten, that the very first epidemic of yellow fever of which we have the records, occurred in the southern hemisphere—at Olinda, in Brazil—where it is said to have exercised its baneful effects from 1687 to 1694;[4] and, agreeably to the fanciful notions of the importationists, to have been communicated to vessels from Siam,

[1] De la Condamine, Voyage à l'Equateur, Paris, 1751, iv. Ulloa, Voyage Historique de l'Amérique Méridionale, ii. 149 (1752).

[2] Leblond, Observ. sur la Fièvre Jaune, 200.

[3] Humboldt, Essai Politique, iv. 503, 8v. ed.

[4] Ferreyra da Rosa, Tratado unico da constituiçao pestilencial de Pernambuco, em Lisboa, 1694.

which in their turn introduced it into Martinique. I am aware that doubts may be entertained as to the real icterode nature of that fever, and I myself am not free from misgivings on the subject; but, admitting it to have been nothing more than a severe form of bilious remittent fever with typhoid tendencies, some of the cases observed on that memorable occasion presented the decided characters of true yellow fever, thereby showing the possibility of the occurrence in question. Since then, we learn from Sigaud that, during his residence at Rio, he saw five well-marked cases of the disease.[1] Add to this that the events of the last few years are of a nature to convince the most skeptical of the possibility of true yellow fever occurring in that hemisphere; for no one is ignorant of the fact that the fever broke out at Bahia, in Brazil, and soon after at Rio Janeiro, where it pursued the epidemic course it pursues everywhere.

But these facts, to say nothing of the circumstance that the disease observed by Fontana[2] and others on the East India coast, presented the characteristic marks of yellow fever, will be sufficient to disprove the statements referred to, and to convince us that the same causes which occasion the disease on one side of the line may do so equally well on the other. If, therefore, we find the disease less apt to prevail in some places than in others, and if a like exemption as regards the other forms of malarial fevers is noted in various localities, we are not from that circumstance warranted in denying the febriferous power of malaria, for, on examination, it will be found to arise from the operation of well-known influencing agencies.

The exemption may be explained in various ways.—Sometimes it is due to the high elevation above the level of the sea of the place so exempted. At another time, the effect is attributable to the absence of a sufficiently high and long-continued atmospheric heat. In other instances, the circumstance is due to a very perfect and constant ventilation, and a very rarefied and pure character of atmosphere. In some instances, again, it may be explained by the peculiar geological character of the soil; the quantity and the quality of the surface-water; or the proportion of sulphates the latter

[1] Du Climat et des Maladies du Brézil, 258.
[2] Des maladies qui attaquent les Européens dans les pays chauds, 72–3.

contains in solution. Sometimes, also, it is due to the rapidity of the river currents, the excessive and rapid dryness of the atmosphere during the hot season, the existence and extensive prevalence of refreshing and purifying winds, and often to the degree of desiccation the surface has attained by natural or artificial means, the degree of cultivation to which it has been carried, and other agencies of like import, as well as by the extent to which it is sheltered, by rich foliage and other means, from the action of the sun. So far as ships are concerned, the freedom from fever will often be found ascribable to the latitude in which they may be navigating, to the early period of the year in which they may be at sea, or otherwise employed; or, to the absence of an epidemic constitution of atmosphere.

In the examination of the subject, none of these contingencies should be overlooked. Experience has shown that there is an altitudinal range, varying in different parts and according to the peculiar form of the disease, beyond which, owing to the greater rarefaction of the air, peculiarity of temperature, or other circumstances, the elimination of the febrile poison does not take place, or the latter is rendered inert; that a certain range and continued elevation of the thermometer is required for its development; and that free ventilation, and strong unimpeded currents of wind are inimical to its morbid agency. Experience has shown in addition, that while, as we have seen, an argillaceous soil is prone to the development of fever, a region of primary formation, with a sandy, calcareous, arid, and sterile soil, allowing no stagnant water, and containing only a small proportion of organic remains, is usually exempt from the disease; that, in not a few instances, the passage from a fever to a healthy locality takes place within circumscribed boundaries, and is indicated by a difference in the geological formation of the soil; and that, if exceptions to this occur, the explanation is easily found in the fact that the cause of the disease has been wafted from some pestiferous region, to a locality which, otherwise, would have remained unaffected; or in the circumstance that the calcareous structure of the sickly spot, though naturally of a kind not subject to fever, is covered over, extensively or in spots, with a more or less thick coating of rich absorbent soil, possesses an argillaceous substratum, or presents depressions, in which water stagnates, and the process of decomposition takes place. Again, experience has shown that water containing a small

amount of sulphates, is less injurious to health, and less prone to favour the formation of malaria, than that which is richer in those materials; while on examination, it is found that the vegetable matter contained in peat moss is subcarbonized, and necessarily unsusceptible of decomposition; that such moss, as already stated, is known to possess peculiar antiseptic qualities, which, by imparting to it the power of preserving not only trees and other vegetable but animal substances from putrefaction, renders it unfit to evolve the efficient cause of periodic fevers.

See the remarkable contrast noticed in the relative prevalence of agues in New England, New Brunswick, and Nova Scotia, on the one hand, and on the other, in the region of the great lakes on either the American or British soil. This contrast, resulting from the absence of such fevers in the former region, and its great prevalence in the latter finds no explanation, as Dr. Forry well remarks, in any difference of climate as regards temperature and moisture; but the solution must be sought in the modification of climate arising from geological formation and the nature of soil. "Now, as the region of New England, as far as the St. Lawrence, with little exception, has a primitive formation, with a sandy, sterile soil, whilst that of the lakes consists of a secondary formation, having, not unfrequently, an alluvial superstratum, composed of a rich vegetable mould from three to six feet deep, it is not difficult to deduce the correct inference. In the former, the geological structure is destitute of organic remains, and the little contained in the sandy soil does not find enough moisture to induce the necessary chemical action; while in the latter, not only is the geological formation of secondary origin, but the deep, rich soil is sufficiently humid, when a high temperature acts upon the organic remains with which it abounds, for the development of the morbid poison called malaria."[1]

The fact that some marshy surfaces, which are supposed to give rise to fevers by means of the exhalations issuing from them, prove completely, or to a great extent, innocuous in certain seasons, and even during a succession of years, cannot be urged as an argument in favour of the denial of the malarial cause of fever, finding, as it does, a ready explanation in the absence, during those seasons of exemption, of the thermometrical and hygrometrical

[1] Forry, Climate of the United States, 280.

conditions, which experience has taught us to regard as essential to the evolution of the poison, as well as, often, in the absence of that peculiar state of atmosphere to which the name of epidemic, constitution, or meteoration has been applied; and the agency of which, in diffusing and enhancing the virulence of the cause of certain diseases—explain it as we may, and extraordinary as it may appear—it would be just as reasonable to doubt, as to doubt the existence of those fevers themselves.

Elevation.—In regard to elevation—not that to which the cause of fever, and of course fever itself can be carried, for we shall see that much may be done in that way;—it is a fact of common notoriety that it exercises a marked influence, not only on the form and type of the disease, but even on its very existence; an influence not very dissimilar from that which, as already pointed out, is occasioned by geographical latitude. If fevers decreased in frequency in proportion as we proceed in a northern direction; if at last we reach a latitude where they cease to manifest themselves; so, in like manner, as we ascend from the level of the sea, they are found to diminish in frequency till at last we arrive at a degree of elevation varying according to many concomitant circumstances, where localities which, in lower regions, would be sure to be more or less infested with malarial fevers may be visited and inhabited with entire impunity.

We have already seen that the marshes on the summit of the Calabrian Mountains do not possess febriferous powers. In other parts of Italy, though some places, as Sezza, Norma, Sermonetto, at an elevation of from 230 to 300 metres, are unhealthy, Monte Mario, near Rome, at a height of between 130 to 150 metres (about 495 feet), and Tivoli, at one of 208 metres (about 690 feet), are free from the fever which scourges the neighbouring localities.[1] Much, of course, will depend on the direction of the wind, the mode of exposure, and other local circumstances. As a general rule, according to Tournon, when we reach a height of from 120 to 150 metres above the level of the plain, we enter the salubrious zone,[2] whatever may be the nature of the localities so situate. Monfalcon (p. 75) agrees with those who fix the limits at four or five hundred metres

[1] Tournon, Etudes Statistiques sur Rome, i. 209; Julia, 25, 26; Breyslack, Voy. dans la Campanie, ii.

[2] Tournon, *ibid.* 209; Carriere, 314 (note).

—1,167 to 1,667 feet. The Italians are well aware of this advantage of elevation; for, according to them, in ascending, we pass gradually from the aria *passima* to the *cativa;* thence to the *sospetta;* then to the *sufficiente;* to the *buona;* and, finally, to the *fina o ottima.*

The mountains of Corsica,[1] of Tyrol,[2] and of Switzerland,[3] are in like manner exempt from the same diseases, while the valleys and low lands are severely afflicted; and yet, in the former, sources of marsh exhalations are not wanting. Maroon Town and Phœnix Park (Jamaica), each at a height of 2,000 feet, are remarkable for healthiness.[4] The inner Cabrite and the outer Cabrite—the first at 430 and the latter 590 feet of elevation—have also been found very healthy. In Grenada, Morne Cardigan, 500 feet, and Richmond Heights 730 feet, are not sickly.[5] Mount Demoulin, near Roseau, in the Island of Dominica, at an elevation of 1,500 feet, has been invariably free from yellow fever.[6] The same immunity has been noticed in St. Domingo, in the mountainous parts of which, whatever be the condition of the soil, this disease does not prevail.[7] The same fever has made its appearance, in 1812, and other occasions, on Brimstone Hill, St. Christopher, at an elevation of 700 feet,[8] and four times (1817, 1825, 1827, and 1831) at Stoney Hill, Jamaica, the height of which is 1,360 feet.[9] But these are generally healthy, and free from ordinary fever. In Mexico, according to Humboldt, the farm of the Encero, the height of which is 928 metres, forms the superior limits of the vomito,[10] and the same disease scarcely ever passes beyond the ridge of mountains that separate La Guayra from the valley of Caraccas.[11]

Major Tullock, while remarking that mere elevation to a height of 600 or 700 feet, does not secure a healthy position, as demonstrated by the instances of Fort St. George, at Toboga, of Morne Fortuné, at St. Lucia, and of Mount Bruce, at Dominica, where, indeed, the results were the reverse of salutary, adds that it is proved beyond a doubt that, "at an elevation of 2,000 or 2,500 feet,

[1] Heurteloup, Tr. de Giannini, i. 269 (note).
[2] Zimmerman, de l'Experience, i. 106.
[3] Bosquillon, Tr. de Cullen, i. 76.
[4] Imray, Ed. J. lxx. 260; Arnold, 192; Statist. Rept. of Sickness, &c. 63.
[5] Hunter, 307.
[6] Imray, Edinb. J. lxiv. 340; Lind, 224.
[7] Bally, 326, 335; Dalmas, 64, 65; Gilbert, 102.
[8] R. Jackson, Sketch, i. 16.
[9] Tulloch's Rept. 59.
[10] Nouvelle Espagne, 771.
[11] Humboldt, Pers. Nar. iii. 392–5.

they are likely to be wholly exempt from that disease, or to encounter it in so very modified a form, that the mortality from all causes will not, on the average of a series of years, materially exceed that to which an equal number of European troops would be subject in the capital of their native country."[1] Bally also gives to the yellow fever an analogous altitudinal limit.[2]

In this country, the yellow fever is never known to prevail in very high situations, whatever may be the condition of the localities. As regards ordinary autumnal fever, the exemption is noticed from one end of the continent to the other. The city of Mexico, and the surrounding country, is never visited by the fever which scourges the land near the level of the sea. "The inhabitants among the sources of the Kenawba and Tennessee Rivers, on the Appalachian Mountains, at a medium elevation of nearly three thousand feet, are almost exempt, while those who occupy the valleys under the same parallels are affected; and farther north, at half that elevation, where the Alleghany and Genesee Rivers have their sources, the disease is almost unknown, while on the shores of Lake Ontario, directly north, it prevails. Finally, the constantly increasing elevation of the desert to the west of the Mississippi is, no doubt, one cause of the disappearance of the fever under the same parallels in which it prevails on the banks of that river."[3]

Assilini, in his treatise on the Plague, remarks that, during all the epidemic fevers, and even the most dreadful plagues, there have been, in those cities and provinces where such diseases were raging, some healthy spots. "The citadel of Caïro presents," he states, "one example. It has been observed that the inhabitants of this fort and its environs have always escaped from the plague, even from that of the year 1791. If the inhabitants of this fort, in spite of their daily intercourse with those of the city, were preserved from this disease, it must be because the damp and infected air which had destroyed the health of the inhabitants of lower Caïro, had not sufficient elevation to reach to the highest part of the citadel and its environs, and consequently could not impair the health of those who lived there." (Pp. 58–9.) Just as probable is it that the exhalations giving rise to that form of fever cannot be eliminated in that high and salubrious situation. In the year 1835, when the

[1] *Op. cit.* 103. [2] Typhus d'Amérique, 325, 6. [3] Drake, 715.

disease committed such ravages in the same city, the citadel was again preserved, as well also as the village of Loumeldik, the situation of which is sufficiently high to overlook the whole Peninsula.[1]

As regards the cause of the diminution and cessation of miasmal fevers at certain degrees of terrestrial altitude, it will be unnecessary to enter at any length in this place. The effect may be due in some measure to a diminished atmospheric pressure. But many circumstances induce the belief that the explanation must be sought principally in the diminished or low temperature which forms a characteristic of high localities; for it is a well-ascertained fact that, except in cases where the soil expands into extensive plains, and the irradiation of caloric which ensues counterbalances the cold incident to such positions, the elevation of the soil is invariably attended by a diminution of temperature. It has been computed that a perpendicular elevation of from three hundred to four hundred feet produces a decrease of heat equal to that attendant on an approach of one or two degrees toward the poles. Fuster, in his highly interesting work on the *Diseases of France*, calls attention to the fact that, under the line, a degree of cold is generally found to correspond to an elevation of 219 metres or 730 feet; in the temperate zone, to 174 metres or 580 feet; in winter, to 70 metres or 233⅓ feet less than in summer; and at seven o'clock of the morning to 60 metres or 200 feet less than at five o'clock of the afternoon. In Paris, during the hot season, when the ground is nearly as much heated as it is in tropical regions, it was ascertained by Gay-Lussac, at the time of his aerostatic ascension, that at a height of 7,000 metres, or 23,333⅓ feet, a degree of cold corresponded to an elevation of only 174 metres or 500 feet.[2]

Supposing these statements to be correct, it follows, as M. Boudin remarks, that, at the 46th degree of N. latitude, an elevation of 2,000 metres or 6,666⅔ feet, would give us the temperature of Laponia.[3] With this before us, we can understand that febrile complaints, which are found to diminish in point of frequency as we proceed in a northerly direction, and finally disappear when we reach a certain point, will equally cease to exist at a given height above the level of the sea.

[1] Clot Bey, De la Peste, 223; Brayer, Neuf Années à Constantinople, i. 357.

[2] Fuster, Des maladies de la France dans leurs rapports avec les saisons, &c. 33.

[3] *Op. cit.* 35.

This will appear the more natural when we take into consideration the effect of temperature on the production and dissemination of these complaints, for, as Dr. Forry has well remarked, the meteorology of heat is intimately connected with the laws of malarial diseases in the relation of cause and effect.[1]

Degree of heat.—Experience teaches us that, although heat alone cannot be viewed as the efficient cause of autumnal or periodic fevers, a certain range and permanence of elevation of temperature are necessary to insure the development of the disease; and that, unless this obtains, the latter does not make its appearance, however favourable in appearance the soil and localities may be to the production of morbid exhalations. It has already been seen that fever rarely prevails in cold climates, and then only during hot weather; that in temperate latitudes it never shows itself in the winter season, but breaks out towards the middle of summer; that a temperature of sixty degrees is necessary for its manifestation; that it will not prevail as an epidemic where the temperature of that season falls below 65°, and that it disappears on the accession of frost.[2] The necessity of a still higher temperature, continued during a certain time, has been found requisite to insure the production of true yellow fever; for, whatever be the condition of the localities where it generally breaks out and prevails, the occurrence fails unless the thermometer marks a high degree of heat, and continues to give a certain average during weeks or months. We all know the average summer temperature of the tropics—the proper soil of the disease. Nowhere, whether in Africa, the West Indies, or on the South American coast, does it fall under 80°. In our Southern States, where the winter season is characterized by cool weather, the summer heat approaches to that of tropical regions; and when the fever, which, as is known, is not there of annual occurrence, breaks out, it does so generally in seasons when the range of the thermometer equals that of the West Indies and Vera Cruz. Such has been the case at New Orleans, Mobile, Natchez, Charleston, and Savannah.[3]

[1] Climate of U. S. 277.

[2] Drake, 715.

[3] Gros, 5; Thomas, 62, 78, 109; Barton, Fev. of N. O. in 1833, pp. 4, 5; Baxter, Med. Repository, xxi. 3; Rept. on Fever at N. O. in 1819, 6, *ib.* for 1839, Journ. of Med. Soc. 156; *ib.* for Fev. of 1820, for 4; Shecut Med. Essay, 77, 93, 103; Tooley, Fever of Natchez, in 1823, p. 7; Girardin, 51; Merrill, N. A. J. ii. 237, *ib.* Med. and Phys. Journ. ix. 233; Monnett, 11, 35; Chabert, 23; Perlee, Med. and Phys. J. iii. 17;

Nor is this less the case in our Middle States, where the summer temperature likewise approximates closely to that of the torrid zone, for there the fever has scarcely ever appeared, unless the mean temperature has reached or approached to 80°. This has been observed at Norfolk, Baltimore, New York, Providence, and Boston. It was also early pointed out in this city, and has been noticed in all the epidemics that have prevailed here from 1699, to the present year. All this the reader will easily find by referring to the writings of Valentin,[1] Whitehead and Selden,[2] Archer,[3] Dalmas,[4] Miller,[5] Seaman,[6] Bayley,[7] Townsend,[8] Drysdale,[9] Wheaton,[10] Brown,[11] Cadwallader Evans,[12] Caldwell,[13] Deveze,[14] Chapman,[15] Folwell,[16] Ffirth,[17] Rush,[18] and others.[19] Sir Gilbert Blane has called attention to the same fact, in relation to the yellow fever of Europe,[20] and his observations are fully confirmed by those of every writer on the epidemics of Cadiz, Barcelona, Gibraltar, Leghorn, Xeres, Seville, Carthagena, Marseilles, &c., where the appearance and prevalence of the fever has, as elsewhere, invariably been associated with a mean temperature, in every way equal to that observed during sickly seasons within the tropics.[21]

Cartwright, Med. Recorder, ix. 6; Dickson, Med. and Phys. J. iii. 251; *ib.* Eclectic J. iv. 112; Simon's Address, 3; *ib.* Rept. 10; Chalmers, Climate of S. C. i. 164; Moultrie, French Translation, 8; Ramsay, Hist. of S. C. ii. 83; Lining, Edinb. Essays, ii. 409; Dupré, Am. J. (N. S.) ii. 382; Waring, 20; Tucker, Barton's J. ii. 22; Fenner, N. O. J. Sept. 1848, p. 194.

[1] Fièvre Jaune, 85, 6.
[2] Med. Repos. iv. 129.
[3] Med. Recorder, v. 61.
[4] Fièvre Jaune, 30, 38.
[5] Works, 118, 119, 433.
[6] Webster's Collection, 1, 2.
[7] Fever of N. Y. in 1795, p. 124.
[8] Fever of N. Y. in 1822, p. 259.
[9] Med. Museum, i. 31.
[10] Med. Repos. x. 329.
[11] Med. Repos. ii. 360, 467; Treatise on Fever of Boston, in 1798, p. 26.
[12] Eeclect. Repertory, vii. 425; and Med. Recorder, i. 139.
[13] Med. Mem. (1826), 87.
[14] *Op. cit.* 116.
[15] Med. and Phys. Journ. viii. 356.
[16] Fev. of Phila. in 1798, pp. 13–22; *ib.* 1797, p. 48.
[17] Treatise on Mal. Fev. 21, 2, 3, 4.
[18] Works, iii. 41, 49, 120.
[19] Emlen, N. A. Journ. v. 329; S. Jackson, Fev. of 1820, pp. 12, 13; Watts, N.Y. Med. Reg. 30; Carey, Fever of 1793, Meteor. Tables; Med. Repos. ii. 406; Currie, Fever of 1793, p. 15; Smith, on Epidemics, 78; Currie, Fevers of 1798, pp. 2, 4, 5, 37, 8; *Ib.* 1798, p. 29; Hill, Fevers of Wilmington, N. C. Recorder, v. 87; Hardie, Fever of N. Y. in 1798, end of vol.; Revere, Fever of Baltimore, in 1819, Recorder, iii. 217.
[20] Dissertations, ii. 155.
[21] Fellows, 33, 244, 565; Amiel, in Johnson, Trop. Climates, 250; Pariset, Fièvre de Barcelone, 12, 185; Robert, Guide. Sanit. 110, 111, 742; O'Halloran, 122; Burnett, Fever of Medit. 205; Tommasini, sect. 161, ii. 483; Bally, 323; Rayer, 23; Roch-

The necessity of a high degree of atmospheric heat for the extrication or production of the febrile cause out of the materials from which it is usually found to be obtained, is every day illustrated in Italy, for there, as elsewhere, marshes or collections of putrescent substances, which are, to all external appearances, in a fit state to evolve the poison, and even to emit vaporous effluvia offensive to the olfactory nerves, remain innocuous so long as the thermometer does not reach a high point. "Thus," says Thouvenelle, "in the marshy lagunes of Venice, during the very low tides of January and February, the air of the capital is infected with an hepatic and miry odour during ebb-tide. Nothing is seen at the bottom of the canals of the city, and all around the latter, but fetid and black mud. And yet, notwithstanding, the health of the inhabitants was not seen to be affected in a way to justify its being attributed to that cause."[1]

"If the temperature," says Dr. McCormack, "prove habitually below 80°, whatever be the exuberance of vegetation or the quantity of marsh land, malaria is never generated. Malaria does not subsist in the north of England, nor in Scotland, nor in Ireland, nor in Sweden, Denmark, or Norway, nor in the colder parts of Europe, Asia, North and South America; in fine, nowhere, so long as the temperature—a few degrees more or less—keeps below 80°. Let it rise above this, and thereupon we have malaria, and periodic fever in all its forms." "If the summer temperature in Ireland ranged habitually from 80° to 100°, paludal fever would prove even more frequent and destructive than ever continued fever was known to be. In point of fact, some low-lying ranges of the uncultivated land near Dublin and Belfast, were said, during hot summers, thirty or forty years ago, to be productive of malaria; but there is nothing of the kind now, and intermitting fever is rarely witnessed in Ireland, except in persons who have been abroad, or the poor serfs who go to reap the harvests in the marshes of Essex and Lincolnshire."[2]

oux, 110, 111; Audouard, 44; Arejula, 132, 133, 134; Lind, 91; Berthe, 154, 324; Vance in Pym, 64; Tullock, Rep. on Sickness of Troops in Medit. 4, D.; Caisergue's, 18, 19; Pierquin, Trad. de Mem. &c. sur la F. Jaune de Barcelonne, 40; Blin, Trad. du Rapport sur la Mal. Epid. &c. Cadiz, 1800, p. 4; Doughty, Observ. on Yellow Fever, 180, &c.

[1] Climat de l'Italie, iv. 212.

[2] McCormack on Malaria, Edinb. Med. J. lv. 372.

Sheltering from the action of the sun.—Much of the baneful effects of marshes and fresh surfaces are prevented, especially in temperate latitudes, by their being sheltered from the action of the sun. Mr. Fleurian de Bellevue, in a communication to the Academy of Sciences, shows, as the result of his observations in France and southern Italy, that when marshes are well furnished with water, and covered with trees planted very close to each other on the banks and causeways, so as to shelter them from the action of the sun, they are as innocuous during summer as the best soil, well dried, and in full cultivation. The mortality, in such localities, is 1 to 42, or 46; while, in other places, where marshes are dried, and the soil of these is argillaceous, compact, horizontal, and divested of trees—as, for example, in natural prairies that are very dry during summer, and on which rain water remains stagnant some days, the loss is 1 in 25, 20, or even 16. This is conformable to the results of experience in this and many other countries, where it is found that marshes, or new soil, which are completely sheltered from solar heat by a full growth of woods and thick foliage, may remain perfectly or comparatively healthy. It has long been found, as we shall have occasion to show more clearly as we proceed, that in the midst of compact forests, the sun never reaches the surface, its rays being fully intercepted by a thick growth of trees, cypress, juniper, magnolia, reeds, &c. In such places, however apparently calculated they appear to furnish morbid exhalations, intermittents, nevertheless, do not prevail. Dr. Williamson, who, like many others, has called attention to this fact, remarks that families who live in the Dismal Swamp of North Carolina, employed in making shingles, without a perch of clear or dry ground, enjoy more health than people who live on their new plantations, near the river or swamps.[1] That the effect is justly attributable to the cause mentioned, may be inferred from the fact that, on the removal of the means of shelter, fever makes its appearance. But of this more hereafter.

Free ventilation.—Again, experience has shown that free ventilation, and strong unimpeded currents of wind, are inimical to the generation of malarial exhalations, consequently to the production of fever; that where the wind blows freely and strongly, or finds no obstacles from surrounding objects, and where, in consequence,

[1] Hist of North Carolina, ii. 192.

the air is quickly renewed, localities which otherwise might be expected to be fruitful sources of fever, may be visited or inhabited with impunity, while similar places become insalubrious, if the air is stagnant. "Calms," says Dr. Drake, "permit the exhalations from foul localities to accumulate in the atmosphere which rests over them; but all winds operate to disperse and dilute them with purer air; in doing which they may promote the salubrity of one spot and diminish that of another."[1] It is of daily observation, especially in warm climates, and in hot seasons in temperate countries, or in localities subject to periodic fevers, that these assume a severe character, and even spread epidemically when the air has long been undisturbed by winds or thunderstorms. The late Professor Hallé, long one of the magnates of the Medical School of Paris, in an able report on the condition of the River Bievre, near that city, pointed out the fact that the pernicious effects of the fetid exhalations issuing from the banks of that river, are harmless in situations where the atmosphere circulates freely and is renewed by strong and unimpeded currents.[2] This injurious effect of a stagnant state of the atmosphere was noticed as early as the time of Hippocrates, who alludes to it in the third book of his epidemics, and has been dwelled upon, on just grounds, by many writers on the fevers of the West Indies, of South America, this country, and Europe.[3]

In many parts of country—India, and other hot regions, for example—fever prevails widely where it should least be expected; on high mountains. But these are covered with lofty woods, or thick jungles. There the surface of the earth is constantly strewed, particularly in autumn, with organic remains, and kept moist by rain or dews. The lower stratum becomes impregnated with effluvia,

[1] *Op. cit.* 587. [2] Mém. de la Société de Médecine, x. 78.

[3] Le Blond, 7, 16, 19, 106; Clark on Long Voyages, i. 5; Imray, Edinb. J. liii. 92; Lempriere, i. 17; Rufz, 31, 32; Rep. on Sickness, &c. of Br. A. 102; Merrill, Med. and Phys. J. ix. 233; Villermé, an. d'Hyg. xi. 349; Celle, Hyg. des Pays Chauds, 20; Baglivi, Op. Om. 81; N. Y. Med. Repos. ii. 403; Vincent, Fièvre J. 7; Ralph, Edinb. Med.-Chir. Trans. ii. 55, 60; Copland, End. Influences, Dict. i. 759, Am. ed.; J. Clark on Y. F. 49, 56, 57; Monfalcon, 92; Deveze, 134; Caillot, 108; Van Swieten, v. 160; Hancock, 78; Diemerbroeck, cap. 3, 5; Caldwell, Med. Repos. vii. 144, 5; Thouvenelle, *op. cit.* i. 180; Jameson, Dublin Med. J. N. S. xvi. 332; Wilson, Treatise on Fevers, i 79, Am. ed.; Zimmerman on Experience, ii. 391; Ainslie, Smith, and Christy, Fev. of Coimbatori, 60–77; Rep. of London Gen. Board of Health on Sewer Water, &c. 8; Dundas, Sketches of Brazil, 245, 346.

which are seldom agitated by breezes, or rarefied by the sun's rays. Dr. James Johnson, who has noticed this, remarks that among the lofty forests and impenetrable jungles of Ceylon, fever prevails extensively. "It is under the branches of these shrubs," (in Ceylon,) says Lord Valentin, "that the fatal jungle fever is probably generated. Not a breath of air can pass through; and the confined exhalations from black vegetable mud, acquire a highly deleterious quality, affecting both the air and the water."[1]

Many of the bays and inlets in the West Indies, while they derive their security, also derive their unwholesome air through the agency of the hills surrounding them, which gives them an atmosphere little agitated by winds, and in which, consequently, the morbid exhalations from the marshes which are situate along their banks, may well be supposed to continue suspended, accumulate, concentrated by heat, and become infinitely more pernicious. Dr. Chisholm, among others, while calling attention to this fact, and to the correctness of the explanation, adduces several cases in illustration. The French have, therefore, not inappropriately denominated these situations "lieux etouffés."[2] After mentioning that the state of the atmosphere during September, and the first two weeks of October, favoured the accumulation of the miasmata in this city during the memorable epidemic of 1793, Dr. Rush remarks, that the register of the weather shows how little the air was agitated by winds during the above time. "In vain," he says, "were the changes in the moon expected to alter the state of the air. The light of the morning mocked the hopes that were raised by "a cloudy sky in the evening." He very truly observes that, "however inoffensive uniform heat, when agitated by gentle breezes may be, there is no record of a dry, warm, and stagnating air having existed for a length of time without producing disease."[3]

Indeed, all regular and fresh currents of air have the effect of sustaining the healthiness of malarious localities, which, in their absence, would be more or less the seat of febrile complaints. In this country, cases in point might be gathered without the least difficulty as regards common periodic fever; and every physician among us knows full well—for the fact is proverbial—that the yellow fever usually breaks out and is most rife in places noted for

[1] Travels, ii.
[2] Manual of the Climate, &c. of Tropical Countries, &c. 20.
[3] Works, iii. 86, 87.

want of due ventilation, while it requires but a slight acquaintance with medical literature to know, that the same observation has been made in respect to this disease everywhere, and that a similar remark is applicable to the oriental plague. In all places, they prevail and are most rife and malignant in narrow, close streets and alleys, and the poisons to which they are due, if formed, are soon dissipated and rendered inert in open and well-aired situations.

It may be affirmed, without much fear of error, that it is through the agency of the trade-wind alone, which blows almost constantly from east to west, that the greater part of the West Indies is rendered habitable. When this purifying influence is withheld, either through the circumstances of season, or when it cannot be made to sweep the land on account of the intervention of high hills, as is the case on the leeward shore of a portion of Guadaloupe, Martinique, and other islands, the consequences are most fatal.[1] Every one must know that, in tropical regions generally, localities situate in the midst of extensive forests and deep valleys, are, in general, very unhealthy. In the woods, the wind is felt only by the tops of the trees, and it not unfrequently happens that, while the former blows with sufficient violence to break the upper parts of heavy trees, scarcely a breath of air is felt on the surface below. "We cannot form an idea," says M. Celle,[2] "of the fetid odour which exhales everywhere in some of those extensive and damp forests. Scarcely can the sun force its way through the thick foliage; and, when it chances to shine through some opening, the luminous column is almost opaque from the large admixture of vapour and miasmatic effluvia with the atmospheric air. At night, the exhalations increase, and combine with those of the preceding day; for the wind here never replaces the mephitic by a pure air. In the hot season of tropical regions the exhalations attain the maximum of intensity. In this season, too, we find the foliage thickest, and best calculated to prevent the free passage of the wind. Hence, the inhabitants of the forests are then annually decimated by malarial fevers. The same thing occurs in deep valleys, where the winds do not penetrate beyond the superior strata of the atmosphere, and cannot, therefore, renew the inferior ones. Nothing but violent storms, accompanied by heavy rains, can disturb and renew the

[1] Ferguson, Notes and Recollections, 201.
[2] Hygiène Pratique des Pays Chauds, 20–23.

heavy and impure atmosphere. Hence, the soil of the valleys, when not marshy, is healthier in the rainy and stormy season than in April, May, and June, when the predominant winds of the season begin to decline in force, and the air becomes cloudy and calm. As the counterpart of this, I might call attention to the complete or comparative innocuousness of marshy localities exposed to frequent and strong currents of air; and also to the circumstance that the exemption from fever of elevated positions appears evidently due, in great measure, to the free and unimpeded ventilation which there prevails. But this would lead us too far.

Humidity of the soil.—After all that has been said in the preceding pages relative to the share of agency exercised by terrestrial humidity in the production of autumnal fever; of the absence of these when the earth and the dead organic substances by which it may happen to be covered is completely and thoroughly dried; of the outbreak of sickness on the reoccurrence of rain, or of a temporary overflow, by which a certain degree of moisture is furnished; and of the absence or cessation of the disease where the moisture is—from what cause soever—in excess, it is unnecessary to point out, in any detail, the influence which the quantity of surface water exercises in modifying the degree of unhealthiness of marshy surfaces, and regular morasses or swamps. Enough has been adduced to show that the innocuousness of these, on which the opponents of malaria have dwelt with so much complacency, is sometimes due to the circumstance that such localities are so desiccated as no longer to be the source of morbid exhalation; and much more frequently to their being deluged with water, and so completely covered over that the organic matters subject to decomposition are thoroughly soaked, or effectually shielded from the action of the sun. Some marshes are often, or even generally in that condition. Others cease to be the source of infection after heavy showers of rain, or during the height of the rainy season, or immediately after a freshet; and examples are not wanting, in this country and elsewhere, to show that marshy surfaces that are periodically covered under the influence of the rise and fall of the tides, are seldom the source of febrile exhalations.

The foregoing facts will go far to account for the circumstance of the poison contended for not being generated, or if generated not being detrimental to salubrity under all conditions of soil, tempera-

ture, &c. They show that the cause of periodic fevers requires for its development, besides particular materials to be acted upon by external influences, and which abound in certain geological formations and soils of particular kinds, or which are at times collected on the surface of the earth, accidentally or otherwise, a certain degree and continuance of atmospheric heat, and a certain amount of terrestrial humidity—neither too much nor too little; a complete saturation and complete desiccation of the soil or its contents, being alike inimical to the manifestation of the effects to which that cause gives origin.

But these facts, and many others that precede, do more. They lend a powerful support to the opinion which ascribes fever to the agency of a gaseous poison; for they tend to connect this febrile principle with the usual products of the decomposition of organic materials, showing, as they do, that this principle or poison requires for its generation the action of the very same agencies which are necessary for that decomposition. Precisely as takes place relative to the febrile cause, ordinary decomposition of organic matter requires a more or less prolonged continuance and a certain degree of atmospheric heat. Equally necessary is it that there should be a certain amount of moisture. Similarly true is it that an excess of the latter prevents or arrests decomposition; and that a total absence of it will have the same effect. Like the febrile cause, the putrefactive change is promoted by a calm and close state of the atmosphere, and retarded, prevented, or modified by free ventilation and elevated situations. Now, when we find the cause of fever requiring for its development the action of the very agencies which are necessary to insure the development of the gaseous products of decomposition—when we find that without these agencies, applied in certain proportions, neither those gaseous products nor the efficient cause of fever will manifest themselves; and that in all instances in which the latter is produced, as shown by the occurrence of fever, materials capable, when acted upon by the agencies in question, of giving rise to the evolvement of the gaseous products of decomposition—organic matter in various conditions and states of modification exist; and that the total absence of those materials—whatsoever be the degree of heat, and of terrestrial and atmospheric moisture—carries along with it an absence of fever, we can have no reason to doubt the propriety of admitting that the cause of the disease bears a close analogy to the aforesaid

gaseous products; and that if in regard to the former, heat, humidity, and other agencies, acting in given proportions and in concert, on animal or vegetable matters, give rise to the evolvement of certain gaseous substances, the febrile poison, which in like manner requires for its development the action of the same agencies, as also the existence of kindred organic matter, must necessarily consist also of some modification of a similar kind of gaseous substance.

CHAPTER III.

EXISTENCE AND MORBID AGENCY OF MALARIA, CONTINUED.

Occurrences on shipboard prove the agency of malaria.—The medical literature of Europe and this country abounds in facts of a nature well calculated to establish, in a satisfactory manner, the existence and morbific agency of malaria; and in the presence of which it is difficult to conceive how any observing, reflecting, and unbiased physician can refuse to recognize the connection as cause and effect between that poison and fever. When they venture on the denial of that connection, the opponents of the malarial origin of autumnal fevers must forget, for we cannot suppose them ignorant of, the eventful occurrences on board of the United States ships General Greene, Macedonia, Peacock, Hornet, and Enterprise; of the French ships Egérie, Messager, and Euriale; and of the English vessels Bann, Eden, Eclair, Iphigenia, Primrose, Dasher, Dart, Pique, Regalia, Driver, Rattlesnake, Childers, and Pyramus. In each of these, and in many others which could be cited, malignant fever originated and prevailed widely, and was traced in a way not to be questioned, to miasmata exhaled from the contents of the holds or from the timbers of the vessels.[1]

In most of these cases the disease broke out in vessels that had sailed from, or were lying in ports, where, if it ever had prevailed it did not do so at the time, or had not for some or even many years before; and when, therefore, it could not have been derived through the effect of contagion, or the influence of common atmospheric causes; for these, had they produced the effect in question on individuals on board, would not probably have spared those on shore. In some, the fever appeared at sea during a cruise or passage, and

[1] See an Essay on the subject by the present writer, in Am. J. of Med. Sc. for April, 1853.

far from any contaminated spot; and in not a few instances, while the vessels infected were severely visited by the disease, others situate close by, in port or at sea, or on the same cruising ground, and which, had not the cause been located in the vessels themselves, would in all probability have been affected like the former, remained uninjured. That the disease in the vessels mentioned derived its origin from effluvia evolved on board, and not from ordinary atmospheric influences or a contagious principle, we may infer from the circumstance that none of the sick who were landed from vessels so infected and received into hospital wards in various places, were found, in a single instance, to communicate it to those around; no one from on board carried the disease on shore, while nearly every individual who ventured on board sooner or later was attacked. In many instances exposure to the effluvia issuing during the cleansing and purifying process from the hold, of ships heretofore uncontaminated, or very long free from the fever, after the crew had been dismissed, and when no one was left on board to communicate disease, has been, not in tropical and fever regions only, but in temperate climates also, the cause of the most concentrated and fatal form of the malady; while other individuals, who were placed under the influence of the same general morbid agencies as the former, escaped the infection by avoiding such exposure.

Whether the latter proceed from effluvia issuing from the bilge-water, the timber of the ship, the filth of the hold, or the cargo, the existence of the cause within the precincts of the ship—particularly in the hold—is rendered evident not only by the circumstances already mentioned, but by the limited space and the particular spots to which it is confined at the outset, or throughout the whole duration of the epidemic. The disease (yellow fever) usually or very frequently makes its appearance, and is more severe, in the vicinity of the pumps and main hatchway. This is exemplified by the occurrences on board of the General Greene, the Macedonia, the Rattlesnake, the Rainbow, the Force, the Skipjack, the Ferret, the Scylla, the Lively, and the Isis, and is nothing more than what might have been expected; for there the keel is most dependent, the water draining from other parts is accumulated, and the heat is most intense. Thus, in English frigates, the first cases, and the largest rate of mortality, have in many instances been found to occur in the berths of the midshipmen and marines, which, in such vessels, are placed on each side of the pumps and main hatchway. We are

told by Dr. Wilson, that the subsequent progress of the disease depends on the trim of the vessel and the inclination of the keel from the horizontal position—the fever spreading in the most dependent parts. In almost all cases, the disease prevails more, and the risk of infection is greater, in the lower than in the upper deck. It is sometimes confined to one end or one side of the vessel. Take the following as an illustration.

"In the Rattlesnake, the fever was first manifested near the main hatchway; the marines and the midshipmen of one berth, suffered its earliest and most severe effects. Afterwards, it proceeded forward rapidly, but pretty regularly, till it had affected almost the entire ship's company; but it did not go beyond the steerage in the opposite direction, no one being attacked in the gun-room except the purser, and I have good reason to conclude that in his case it was derived from the shore." "But its local origin and limited range of action were most strikingly exemplified in the berths of the midshipmen, and other officers of that class. They were placed exactly opposite to each other, with the pumps at equal distances between them. Only one gentleman was affected in the starboard berth, while every member of the larboard berth was laid up nearly at the same time. The hatch of the pump-well is opposite to, and within three feet of the larboard berth. The members of that berth were generally the junior officers of their class, and were, therefore, it may be said, most susceptible of the disease; but when it is remembered how extensively it prevailed, and how indiscriminately it attacked persons of all ages and temperaments among the ship's company, after every allowance is made for the greater susceptibility of these young gentlemen, the exemption of the others, senior only by a few years, is too striking and complete to be accounted for by their former service, or by any accidents which can reasonably be supposed to have affected them. It can fairly be attributed only to their not having been exposed to the cause of the disease with the same concentration of power, or permanence of operation, as the others; and here the contiguity of the pump-well hatch to the larboard berth cannot be overlooked."[1]

When we bear in mind the particular condition of the vessels in which the yellow fever makes its appearance, as well as the thermometrical and other influences in connection with which it

[1] Wilson, Memoirs of West Indian Fevers, 159, 160.

breaks out and prevails, and compare the results of those various circumstances with such as are obtained on land from kindred local conditions, under the modifying agency of the same meteorological influences, we shall find no valid reason for refusing to admit the relationship as cause and effect between the product of the decomposition which necessarily ensues and the disease which, affecting only individuals exposed to it, spares those who keep aloof. At the same time, we discover a satisfactory explanation of the manner in which these results are brought about. As the disease requires invariably for its production a long continuance of high atmospheric heat, and as it is, on that account, most generally encountered in tropical, or during the hot season of temperate climates, we can readily understand why it more frequently occurs on board of ships in the former. Besides, such vessels usually contain materials susceptible of decomposition, and which, when acted upon by the high temperature they there encounter, are soon productive of noxious exhalations. In merchant and other vessels engaged in the West India, South American, or African trades, the cargoes consist almost exclusively of articles susceptible of fermentation or putrefaction; while the same vessels, as well as ships of war, are apt to be ballasted with materials alike liable to those injurious changes, and which, together with portions of articles provided for food, chips of wood, shavings, and dirt of all kinds that inevitably find their way—sometimes in unaccountable quantities—into the holds of vessels, where, meeting with more or less moisture, supplied by leakage, or the water used for purposes of cleansing or purification, or the drippings of the casks, or other sources, they form a mass which, under the influence of intense heat and a still atmosphere, becomes the source of concentrated miasmata. By one well versed in all matters of the kind, we are reminded that it is not always possible to account for the dirt and rubbish which may be found in the bottom of a ship.

"Fragments of wood, vegetable substances, and dirt of all kinds, however, gravitate by the formation of the vessel towards the kelson or limbers; where, by the heat of the climate and the action of the salt-water, they rapidly decay, and form a blackish mud, not dissimilar to that observed among the roots of mangrove thickets on the banks of the rivers within the influence of the tides; it sometimes even acquires a consistence sufficient to block up the passage of the limbers. In small vessels, with a flying deck, there is less difficulty in accounting for the presence of foreign

matters in the hold; dust, fluids of various kinds, the sweepings and scrapings of the decks, and a thousand other things, will find their way there, notwithstanding the greatest care, both on the part of the officers and men, although the latter, nevertheless, are frequently wilfully careless in this matter." "In steam-ships, the difficulty is still greater, in consequence of the boilers and engines occupying a large portion of the floor of the hold, which cannot be got at. All this filth, in the concentrated state mentioned, may, and often does, exist in vessels, although the decks and other parts apparent to the eye are clean. In them, the cause of offence, though hidden, is not less real than in vessels differently conditioned, and proves often detrimental to health before it is suspected to exist. Of course, all foul ships are not necessarily unhealthy; but those that escape are the exceptions."[1]

Nor is it less worthy of remark that the very timber of which vessels are constructed, especially when green or not perfectly seasoned, may be, and has not unfrequently been, when acted upon by the high temperature of tropical climates, the source of disease —an effect that could not be produced otherwise than through the morbid agency of noxious effluvia resulting from the action of heat on such materials. At any rate, such a condition of vessels has not unfrequently been associated, in hot climates, with the development of the most malignant form of the disease; and, judging from what has been noticed on land of the effect of exhalations from kindred sources of infection, or at sea, from the stowage of damp green wood, we cannot err greatly when attributing the mischief, in the instances in question, to the decomposition of the ship's timbers. On this subject, the facts related by Dickson, Wilson, Rochoux, and others, leave no doubt, or, at the very least, merit serious consideration. The unhealthiness of ships built of green timber has been noticed from time immemorial, and it will be found on examination that severe epidemics have, on various occasions, been traced to the stowage of a quantity of green wood in the hold of vessels. This was strikingly the case in the English war-ship Regalia, for the medical account of which we are indebted to Dr. Ferguson,[2] and in the French brig Messager, referred to by Dr. Rochoux.[3]

We all know, also, how offensive the bilge-water becomes from

[1] Bryson, 223.

[2] Medico-Chir. Trans. viii. 108.

[3] Recherches sur les Différentes Maladies qu'on Appelle Fièvre Jaune, 61.

admixture with the filth of the vessel; or the great tendency that the water, which finds its way by leakage or otherwise into the hold, has of being decomposed; sometimes owing to its original impure state, at others, to its combination with the fresh-water proceeding from the drippings of the casks, &c. It is not to be denied that such a condition of the bilge-water is not necessarily connected with the appearance of disease on board of ships; cases having been adduced in which the foulest and most offensive water has proved innocuous; while disease has raged where the odour was hardly, if at all, perceptible. But such cases, which find their prototypes on land, where localities noted for filthy pools escape, and where the appearance of malarial fevers is sometimes connected with an apparently pure condition of the water partially covering marshy swamps, or with an absence of mud or filth, are not frequently met with in fever seasons and latitudes. More generally the reverse takes place; and even were this not the case, the appearance of the disease may still be due to internal miasmata issuing from other sources of infection existing on board; or, again, the water, though completely or almost inodorous, clear, and apparently pure, may nevertheless contain in solution those particles of poisonous matter which produces the fever. Nor does it necessarily follow, as some writers seem to imagine,[1] that the healthiness of foul ships in southern or other latitudes disproves the opinion of the connection as cause and effect between exhalations from sources of organic decomposition and fever. Of course, all foul ships are not necessarily unhealthy—all vessels containing vegetable and other matters in a state of incipient or decided decomposition, lying in southern ports or navigating southern waters, even in warm weather, are not all expected to suffer from fever. For, besides that, in good logic, a negation can go but little way in disproving the many well-authenticated facts we possess, in favour of a different conclusion, sources of vegetable and other decomposition on board of ships, are under the control of some of the same agencies, which, as we have seen, modify the effects of like sources of contamination on land. They require, before they can generate fever, to be acted upon by high atmospherical heat; that this heat should continue a certain length of time; that the season should be that in which the fever usually prevails; and that there should

[1] Memphis Med. Recorder, ii.

exist a suitable epidemic constitution of the atmosphere. Remove all these contingencies, and foul ships will generally be found to remain healthy. In the contrary case, those that escape are, as Dr. Bryson has remarked, the exceptions.

Be this as it may, the connection as cause and effect between sources of miasmal effluvia and fever, is conclusively established by the example of those vessels in which the spread of the yellow fever was arrested, even in tropical climates, or during the hot season of temperate regions, by a resort to a thorough process of expurgation. In the Dart, Pyramus, Dasher, and Regalia, the history of which has been referred to in detail elsewhere,[1] the fever was arrested. In reference to the first-named of those vessels, Dr. Dickson remarks: "So many people were taken ill, after going on board this vessel, lying guardship at Barbadoes, that it was difficult to account for it, except on the principle of contagion, until the peculiar construction of the ship, viz. her being divided into compartments below, so as to prevent the circulation of air and the stowage of the water in *bulk*, were adverted to, and on examining the large tanks or cisterns, their bottoms were found covered with an offensive slimy mud or deposition."[2] On the removal of some of these causes, the knocking down of the bulkheads, and the cleansing out of the cisterns, the fever was put a stop to.

On opening the hold of the Pyramus, the effluvia which issued surpassed anything Dr. Hartle had ever witnessed, and affected every one exposed to its influence. The quantity of filth which was taken out was sufficient to fill four large mud-boats, consisting of shavings mixed with coal-tar and the water; which, in consequence of the choking of the pumps, had accumulated under the limber-planks. All the cases which occurred during the process of expurgation were easily traced to exposure to this bog; and Dr. Hartle very justly refuses to refer the disease to the influence of English Harbour, inasmuch as other ships of the squadron that lay much longer there escaped the infection. It may not be amiss to remark that the sick of this ship were landed and placed in the capstern of Antigua dock-yard on the 15th of January; that between that day and "the 30th, only eighteen cases occurred; but that on the 31st, six fresh attacks were added to the list, and the disease again appeared with increased violence and malignity." Dr. Hartle adds: "This sudden

[1] Am. Journ. April, 1853.

[2] Rept. in Bancroft's Sequel, 208, 9.

reappearance and violence of the disease induced me to believe that the people had some communication with the ship, which was then undergoing a general expurgation. This, with a little trouble, I ascertained to be the case." Changes were made in the distribution of the convalescents and the rest of the crew, and the disease was put a stop to completely. The crew re-embarked on the 14th of March in excellent health, and remained so.[1]

On taking up the limber-boards of the Dasher, says Dr. Hartle, "the noxious effluvia surpassed anything that I had before experienced, and it was with difficulty that the blacks, who were accustomed to this work, could remain. The ship-carpenter, who had been constantly accustomed to work in the dock-yard, and on many such occasions, assured me that he had not before experienced so putrid a smell from any ship's hold. Between the timbers there was a collection of carpenters' shavings, &c., in great quantities. These had so completely choked up the limber-holes that the water could not pass to the well of the pump, and lay stagnant. The vegetable matter was, therefore, in a state of decomposition, and this, acted on by high atmospheric temperature, became neither more nor less than a marsh in the centre of the ship." "That the fever," continues Dr. Hartle, "was generated on board, by noxious effluvia received into crowded and badly-ventilated berths, is, I think, fully proved; for the moment the crew and marines were removed from the sphere of this hidden enemy, the disease ceased." Nothing like the most distant appearance of contagion could be traced; for none but those residing on board the ship, or exposed to the effluvium from her hold, prior to her expurgation, suffered by the fever.[2]

That the cause of the fever resided in the hold of the Regalia, is proved by the fact that the cleansing of that part proved particularly injurious to those exposed; and that, while the fumigation of the ship proved of no avail in purifying it, the fever ceased when she was completely cleansed, and, with her hatchways closed, her whole hold had been exposed to the concentrated heat of many stoves. In the case of the Trinidad at Barbadoes, mentioned by Dr. James Johnson,[3] the fever disappeared as soon as the hold was washed and cleansed, the dirt, filth, and stagnant water removed, and scuttles

[1] Facts and Observations in Refutation of Sir G. B. Blane's Doctrines as to the Contagion of Yellow Fever, by A. Musgrave, Appendix B., Med. Ch. Rev. and Journ. iv. 774.

[2] Musgrave, *op. cit.* 774.

[3] Trop. Clim. 164, 1st ed.

cut for better ventilation. To this, let me add that some officers, by the early adoption of proper hygienic measures, particularly of those calculated to insure cleanliness and prevent the accumulation of sources of impure exhalations, have succeeded in guarding their vessels, under the most unpromising circumstances, against the development of fever. While other ships were sorely visited by the disease, Captain Smith, of the British Navy, never had it in those under his command, during long service in the Mediterranean. Like him, many other commanders have preserved their crews by having "the hold of their vessels washed out daily by means of plugs, till the water came out perfectly clear, so that an accumulation of filth could not take place."[1]

Here, then, we have proofs as positive as can be desired;—limitation of the disease to the precincts, or to only one part of some vessel;—infection by going on board;—escape by remaining aloof;—great mortality among the crew or passengers, and immunity among those of other vessels close by;—the existence in the former of materials in a state of decomposition, or of green wood; and their absence in the latter;—the arrest of the disease by the adoption of a proper system of expurgation, and lastly, its prevention by proper purifying measures. We perceive that yellow, or malignant fever—the disease produced on those occasions, and which constitutes one of the forms of autumnal and epidemic pyrexiæ—is the result of effluvia; and certainly, if one of the varieties of these effluvia can give rise on shipboard to one form of such complaints, and that form too the most formidable, we may justifiably infer that the same form of fever, when it breaks out on land, does so through the agency of similar effluvia; and, as a natural consequence, that other and milder pyrexial complaints must be the morbid products of causes, which, though not of an identical are of a kindred character.

The cause of fever wafted by winds passing over sickly localities.—But we are not restricted to this inductive process for materials with which to establish or fortify the position here assumed respecting the malarial origin of autumnal fevers. Facts for that purpose may be gathered from all parts of the habitable world, and have been recorded from the remotest period to the present day, in the history of the disease.

[1] Lond. Med. Geog. ix. 890.

Without dwelling anew on the circumstance that fever abounds more or less in places where the soil is low, alluvial, flat, humid, and filled with organic matters in a state of decomposition; and, on the other hand, that it is little, if at all known, where contrary characters prevail, I may call attention to the fact that certain localities—rural districts, towns or cities—which themselves do not contain any of the elements of unhealthiness, and, under ordinary circumstances, remain free from autumnal or periodic fevers, become, at times, the seat of the latter, when so placed to the leeward of infected districts as to receive from these, through the agency of the wind, the pestiferous miasmata evolved from their marshy surfaces. Facts, indeed, might easily be accumulated to show that, in many fenny portions of this globe, each locality has its favourable and its unfavourable winds.[1] With the latter, fever abounds; with the former, it diminishes or disappears; and it is found that this difference depends on the position of the marshy or infectious surface relative to that locality, and also on the circumstance that, in the one case, those winds pass over the source of the effluvia before reaching the sickly place; while, in the other, they take a contrary course, and leave the latter untouched. I might dwell on the case recorded of Empedocles, who, having observed that the sterility of the soil, and the plague from which Sicily so frequently suffered, were caused by a southerly wind, which reached the suffering district through the interstices of the neighbouring mountains, directed them to be closed, and, by this means, put a stop to the ill effects in question.[2] But it will be more satisfactory to cite a few better authenticated instances which have been noticed in more modern times.

On the west side of the town of Marenne, in France, are situated vast marshes. When the wind blows from the north, north-east, or east—in other words, in such a way as to preserve the town from the miasmata issuing from those marshes, fevers are rarely encountered. When reversely, the wind blows from the west, south-west, or south, *i. e.* so as to pass over those paludal surfaces before reaching the town, fevers make their appearance in the latter. On the contrary, at Saint Agnant, situate opposite to Marenne, and on the other side of the marshes, matters take a different turn.

[1] Melier, Mém. de l'Acad. de Méd. xiii. 671; Jaquot, 18–36, 39, 48.

[2] Leclerc, Hist. de la Méd. 93. Sprengl, Hist. de la Méd. i. 244.

There, during the prevalence of the east wind, the town becomes sickly, while it ceases to be so when the opposite currents set in. The city of Rochefort owes its present unhealthiness in the autumn to the circumstance that the S. W. wind, which predominates at that season, passes, before reaching it, over the extensive and pestiferous marshes of Brouage.[1] The S. S. W. wind, by passing over the basin of the Anthion, opposite to Corné, in the department of Marne and Loire—which, in 1843, was, from a variety of circumstances, in a fit condition for the elimination of miasmal exhalations—occasioned that year a pestiferous infection in all the neighbouring localities exposed to its influence.[2] At Bayniere, in Normandy, an unusual course of N. W. winds, blowing across the marshes, and conspiring with a hot summer, caused very severe epidemics in 1809 and 1810.[3]

The Velabrum, at Rome, the district of St. Peter, the surface extending from the Tiber to the foot of the Janiculus, and the Trastavere generally, from the southern extremity of that mount to the dependencies of the Vatican, are the only unhealthy spots of that district, and are evidently indebted for the fever to the poison which, exhaling from the site of the ancient city, reaches them with facility through the agency of the south winds. These are unimpeded in their course thence by hills or other similar obstacles, while they find no means of escape when once they have arrived there.[4] Lancisi,[5] Baglivi,[6] Rigault de l'Isle,[7] and other writers may be referred to, in testimony of the unfavourable influence of the S. E. wind on the salubrity of those parts of Rome which lie to the leeward of the aforesaid pestiferous region, and of neighbouring marshes; and of the healthiness of other localities, differently situated. The stagnant water of Lake Aguano exhales deleterious effluvia, which are wafted back, in a north-eastern direction, by the opposite wind, on to two villages, and even to the convent of Ca-

[1] Mém. de la Soc. Roy. de Méd. viii. 281. Melier, Mém. de l'Acad. de Méd. xiii. 671–2.

[2] Gaultier de Claubry, Rapport sur les Epidemics, &c. Mém. de l'Acad. xiii. 119, 120.

[3] Macculloch, 357.

[4] Carrière, Climat de l'Italie, 372; see Am. J. July, 1851, p. 163.

[5] De Nox. Palud. Effluv. lib. 2, cap. 3. De Nativis Cœli, Rom. qualitalibus, iv. 157–8.

[6] Prac. Med. lib. i. cap. xv. Op. Om. 80.

[7] See in Johnson Trop. Cl. 315; see, also, Lind on Hot Climates, 30; the same on Seamen, 67; Monfalcon, 77.

maldules, situate a league off, on a high mountain, and there gives rise to fever, from which other localities of the vicinity are exempt.[1] In several parts of Italy, as also in Corsica, France, and many other places, chains of hills, free from morasses, but placed in a line with them, and in the face of a wind which carries to them their emanations, were entirely depopulated and rendered uninhabitable. "Such is the malignity of a marsh lying to the south," says Lancisi, "that, although towns are built upon the tops of hills, exhalations are wafted thither, with all their mischievous properties, by the southern and south-western winds. Gondolpho affords a fact in full illustration of this, for the Lake Turnus lay formerly below it, full to the south. From this the air was vitiated, although the prospect was not injured at all. The ill effects of this were so serious, that Pope Paul V. could correct them in no other way than by draining the lake to dryness. In commemoration of which, the following inscription in marble is to be seen in front of the government-house at Gondolpho: "In the year of our Lord, 1611, Pope Paul V., finding the town to be on the decline from the scarcity of water, and the noxious exhalations of Lake Turnus, contributed to its health and joy by bringing water hither from the distance of three miles, and by drying up the lake, in the seventh year of his pontificate."[2]

The marshes of Mantua, Ferrara, Noverra, Verseuil, afford similar examples of the effects of winds in transferring the cause of fever from one spot to another. In Venice, the north-east wind is healthy, and blows the miasmata of the lagunes on the continent. The opposite winds, on the contrary, waft those miasmata, as well as those of the Lido, and of the mouths of the rivers that open into the former, over the city, and fever, in the exposed districts, is the consequence.[3] On the occurrence of an east wind there is always an increase of febrile diseases in London, which is attributed, in great measure, to the influence of marshes in distant parts. Marsh fever is not unfrequently noticed in the neighbourhood of Tavistock Square, and other places in the hollow of the northern district, which affords free access to the deleterious west winds. The eastern side of Blackheath is invariably afflicted with these winds, and dis-

[1] Valentin, Voy. Méd. en Italie, 45; Cyclop. of Pract. Med. iii. 64; Monfalcon, 79.

[2] Nox. Palud. Effluv. lib. i. cap. v. 20. Monfalcon, 79; Brown in Cyclop. of Pract. Med. iii. 64. Thouvenelle, Climat de l'Italie, iv. 263.

[3] Carriere, 459, 463.

ease is the consequence. A northern wind, blowing over the Essex marshes, often produces agues on the other side of the river. The east wind, which blows from Essex towards London, invariably carries it (fever) on for many miles, as all susceptible persons experience. At the east end of London, it reaches all through Finsbury division and White Chapel, and is even brought at the back of the Strand, along the river.[1] It is a well-known fact, "that in the southern section of the United States, where the prevailing winds of summer and autumn are from the south and west, the dwellers on those sides of marshes, swamps, rivers, and mill-ponds, are often in the enjoyment of good health, while the people on the opposite sides, although farther, perhaps, from the laboratory of the poison, are victims to fever."[2] At Eatonton (Ga.), some thirty years ago, bilious fever carried off many inhabitants. The disease was satisfactorily traced to a mill-pond, about a mile east of the village, remarkable for its filth, and which, having run dry, was exposed to the summer's sun. From this point, the concentrated poison was swept, by easterly winds, to the village. The village of Mount Zion, Hancock County (Ga.), was, in like manner, severely afflicted by autumnal fever. Just previous, an easterly wind prevailed for several weeks, which blew directly across Beaver Dam Creek, nearly a mile distant, whose marshes had become dry under the autumnal sun.[3] During the severe epidemics of 1821 and 1822, in this State, it was observed "that the persons residing on the north and west of streams of water were peculiarly obnoxious to attacks, while those residing on the south side, in the immediate vicinity, were comparatively exempt." The explanation may be found in the fact that, for nearly two months of that summer, the prevailing winds blew from the south, with some slight and short exceptions.[4]

"Since the month of July, 1843," says M. Gaultier de Claubry, in one of his excellent reports on the epidemics of France, "intermittent fevers, which were originally sporadic in the commune of Orville-la-Mer, have assumed the epidemic character, and reached every part of the place, as well the habitations of the rich as those

[1] Edinb. Rev. xxxvi. 542. Second Rep. of the London Commissioners, 1848, p 40. Rep. on the Drainage of the Lands forming the Sites of Towns, 5. London, 1852.

[2] Caldwell on Malaria, 135.

[3] Pendleton, Charleston Med. J. vii. 450.

[4] Rep. of the Burks Co. Med. Soc. in Tr. of Pennsyl. State Med. Soc. ii. 93.

of the poor. The N. N. E. wind, which has continually prevailed, by passing over the pond (situated at some distance), has wafted the paludal miasmata which are there formed into the principal street of Orville, which faces the source of the infectious atmosphere."[1]

The east wind which passes over the marshes of Echats, carries every autumn periodic fevers, often of a pernicious character, to the banks of the Saône as far as Trevoux, at the distance of two leagues from the river.[2]

Intermittent and bilious fevers often prevail at Macuto, and at Caravalleda (South America); and when, from time to time, the sea breeze is interrupted by a westerly wind, the little Bay of Coria sends an air, loaded with putrid emanations, towards the coast of La Guayra, notwithstanding the rampart opposed by Cape Blanco.[3] Again, the situation of the Laguna of Campoma (a great meer, which is divided, in dry weather, into three basins, situate to the north-west of Cariaco, near the extremity of the gulf of that name), renders the north wind, which blows frequently after sunset, very pernicious to the inhabitants of that little town.[4]

At the Naval Asylum, near this city, where fever is of very common occurrence at the usual season, one wing of the building is much more frequently afflicted by it than the other, evidently owing to the fact of its being more exposed to the S. W. wind, which passes over the marshy banks of the Schuylkill River. Among several other instances of the same kind that could be mentioned, I select the following, for which I am indebted to a highly valued friend in Charleston. The hospital, formerly at Fort Johnson, was located on the margin of the marsh upon a high sandy ridge, with a small depression or basin on the western side of it. About three hundred to four hundred yards south-west, were several ponds surrounded by the wild myrtle, cedars, swamp oak, and short-leaf pine. When my informant took medical charge of the station, he learned that his predecessors were of the opinion that the western half of the hospital was unhealthy. Such was the report of the day, and observations subsequently made confirmed the opinion of those gentlemen. For example, it was found that persons placed in the western rooms were attacked with inter-

[1] Mém. de l'Acad. de Méd. xiv. 123.

[2] Groffier, Mém. sur l'insalubrité de la partie Méridionale du Dept. de l'Ain, 19, Foderé, v. 152.

[3] Humboldt, Personal Nar. iii. 399. [4] *Ibid.* 108.

mittents, from which some with difficulty recovered, the disease assuming all the violence of what is commonly known in Charleston as country fever. The observations were made for several seasons, and with the same results. "The hospital was removed to Sullivan Island, and the location occupied by private residences, which are at certain seasons liable to intermittents. The season after the hospital was abandoned by the government, and before its removal, it was occupied by several families of planters from the interior. Those who inhabited the western half of the building were dangerously ill, while the residents of the eastern escaped." "In 1845, a fearful epidemic among children at this settlement (diphtherite or membranous sore throat) made its appearance. In the village of Johnsonville, the mortality was terrible for so small a community; while, on the United States side of this line, there was not a single case of the disease amongst a population of forty-nine children, most of whom were subject to every exposure, and living almost entirely upon salt provisions. This line of malarial demarcation was well defined at Fort Johnson, and the boundary of health and disease could be narrowed down to a very limited compass."

In an account of the medical topography and diseases of Miami County, Ohio, Dr. Volney Dorsey remarks: "The same observation may be made here which is said to hold good throughout the West, that those persons inhabiting the eastern side of streams are more liable to miasmatic fevers than those located on the western border. This, it is believed, is attributable to the fact that most of the streams overflow their banks in the winter and spring, and are almost entirely dried up in the summer and fall, and as at this season westerly winds prevail, the miasm produced by the burning sun acting on the decaying vegetable matter along their course is driven on to the eastern banks. Many lofty and picturesque situations on the eastern side of the Miami River are thus rendered uninhabitable from the yearly recurrence of autumnal, intermitting, and remitting fevers."[1]

Such facts are, indeed, familiar to our physicians—to those especially who practise in rural districts; for they every day find that, during fever seasons, one side of watercourses is generally more sickly than the other, and that the difference depends on the course

[1] Trans. Am. Med. As. v. 450.

of the wind. Dr. Pendleton, to whose essay on the topography and diseases of middle Georgia I have already several times referred, informs us, that he has frequently, in the course of his practice, known fevers to prevail on one side of a creek for days together, while the other was entirely exempt—a circumstance, he rightly thinks, in no way to be accounted for but by the drift of malaria.

In Freetown, Sierra Leone, the agency of the wind in conveying the cause of periodic fevers—remittents and intermittents—is very satisfactorily demonstrated. Here, as in most other sections of tropical regions, these diseases become numerous and frequent after the descent of the first rain. At this period the wind generally blows from the northward and eastward, and consequently bears from the Bullom shore, or the north bank of the Mitomba—the countless stagnant pools and extensive marshy tracts which, loaded with decomposing vegetable matter, have been so graphically described by Mr. Boyle—a large quantum of the noxious vapours therein generated. The direct agency of the wind in conveying these vapours is established by the fact that, at the approach of the dry season, when again the formation of malaria is active, the winds but rarely blow from the swampy Bullom, and fevers are less frequent in their occurrence and decidedly less fatal in their consequences.[2]

Trincomalia (Ceylon)—and very many other places might be mentioned—is never sickly while the north-east monsoon prevails, and does not become so till the south-west wind reaches it from the opposite shore of the island, after having passed over a great extent of low, wooded, and very unwholesome country.[3]

[1] Charleston Med. J. vii. 450.

[2] Boyle, Pract. Med. Hist. Acc. of the Western Coast of Africa, 124, 126; see also p. 44.

[3] Davy's account of Ceylon, 76, 4to.

It not unfrequently happens that the cause of the disease, while wafted to a considerable height above marshy surfaces, and there producing its usual effects, spares, to a greater or less extent, individuals residing in the close vicinity of, and on a level with, the source of the infection; or at least does not affect them more extensively or severely than the former. This has been noticed in various parts of France, Italy, this country, and the West Indies (Monfalcon, p. 80). Experiments made in France, more than half a century ago, leave no room to doubt that the result is due to the upward tendency of effluvia, under the influence of the wind; and of their being arrested in their horizontal migration by the heights they encounter. In the insalubrious localities of the Bresse, white linen sheets were attached to high steeples on the most elevated hills of the vicinity, and an equal number suspended to

Other instances of like import, relative to the effect in question, as observed in Algeria, Batavia, South America, Africa, the West Indies, Borneo, Spain, and this country, are on record, and might easily be adduced.[1] But these must suffice. When taken in connection with those instances which, though generally disbelieved, appear, nevertheless, to rest on respectable authority, of the febrile cause being wafted in the common atmosphere to the distance of several miles—even from Holland to England, as believed by Macculloch[2] and others;[3] of ships receiving the infection at a great distance from land—not less than three thousand feet, according to Sir Gilbert Blane;[4] and of fevers being derived, as already stated, at Rochefort, from the marshes of Brouage, situate four or five miles off;—when, I say, the preceding instances are taken in connection with these facts, as also with the circumstances noticed in Europe, from the frontiers of Asia to the other extremity of that continent, though particularly in Italy, that, as the western coast presents a larger surface of infectious marshes, so malarial fevers, other things being equal, prevail more extensively under the influence of southwest winds than of the opposite currents, they cannot but place the question of the morbific effect, at a distance, of winds passing over pestiferous localities, beyond the possibility of doubt. If we admit

poles ten or twelve feet high, placed in the lowest fields. After the lapse of a certain number of days and nights, all these sheets were carefully examined, when it was found that the latter were merely damp; while the former exhibited black, green, yellow, and livid spots. The experiment was repeated several times, and at various seasons of the year, and always with the same results. (*Ib.* 81.)

[1] Jacquot, Recherches, sur les causes des fièvres à quinquina, 13, 36, 39, 48; Lind, *op. cit.* 113; Osgood, 29; Horsefield, Med. Museum, i. 79; R. Jackson, Sketch, i. 11; *ib.* Outlines, 77; and Treatise, 412; Smelt, Med. Repos. vii. 125; J. Johnson, 84, 93, 153; Leblond, 81; Rush, Fev. of 1780, iv. 232; Evans, 24; Ferguson Recoll. 194; Blane, 252; Davidge, 68; Boussingault, An. de Chimie, lvii.; Rochoux, 113; Bally, 361; Gilbert, 12; Humboldt, 765; Lefort, de la Saignée, 66; Bayley's Letters; Rep. on Fever of N. O. in 1819, p. 50; Waring, 23; Chabert, pp. ix. and 26; McCabe, Ed. J. xv.; Boyle, 44, 75; Pendleton, Topogr. of Middle Georgia, Charleston J. vii. 450; Smith, Rep. to Assembly, on Fever of 1847 in N. Y. 30, Cyclop. iii. 64; B. Perkins, of Boston, Mem. of the Roy. Med. Soc. of Paris, i. 207; Monfalcon, 79; Rand, Med. Repos. ii. 466–8; Fenner, Rep. ii. 442; Bryson, Stat. Rep. of H. of the Navy (Brit.), 11; Second Rep. on Quar. 14, 15; Thouvenelle, Climat de l'Italie, i. 223; Vincent, Dis. sur la F. J. 10; Mabit, Mal. de St. Dom. 5, 6.; Dict. de Méd. Pratique, vii. 74; Volney, Climat. des E. U. 316.

[2] Malaria: an Essay, 93.

[3] Edinb. R. xxxvi. 542; Boudin, Géographie Méd. 70.

[4] Med. Ch. Tr. iii.; *Ibid.* Dissertation, i. 236.

this, we must allow, also, that the effect can only be due to the air thus propelled being the vehicle of some deleterious agent, which it receives from the infected surface, and not to the cold or heat, or the vicissitudes of temperature, or humidity, which such winds may occasion; for these, as we have seen, do not alone produce fevers, which often arise under circumstances rendering such an explanation totally inadmissible. They appear under the influence of the most opposite currents of air. In some cases, a north, at other times a south, or a west, or an east wind, produces the effect —the result depending solely on the exposure of the suffering locality, relative to the position of the source of malarial elimination, and not on any injurious quality appertaining to the wind itself, and existing independently of the extraneous poisonous materials the latter serves to convey; for, as long ago remarked by Schnerrer,[1] wind of itself, whatsoever may be its thermometrical or hygrometrical qualities, must be exonerated from the charge of producing the peculiar morbid effects in question. Sometimes the wind is a humid, sometimes a dry one; and surely, heat and cold, humidity and dryness, cannot all be fruitful sources of a disease which is always fundamentally the same; while the same winds, equally warm or cold, moist or dry, but lacking the extraneous poison under consideration, have blown for months without occasioning fever.

It has frequently been found in marshy countries—and examples of the kind have been noticed from early antiquity in Dalmatia, Calabria, and Sicily—that, by changing the position of towns, houses, and encampments, so as to avoid the effects of winds which blow over marshy or malarious localities, or by preventing access to such winds, by closing carefully the doors and windows of houses exposed to them, fevers have been arrested or guarded against.

Dr. Dundas having noticed that fever attacks were very common among the inmates of the British Hospital, of which he had charge, at Bahia (Brazil), sat about remedying the evil. In the first place, he made several alterations in the hospital, and had those windows nailed up, which admitted directly the current of wind suspected to cause the mischief. At the same time, effectual measures were taken to prevent convalescents from being exposed without clothing. The results of these measures were immediate, and the evi-

[1] Des Epidemies et des Contagions, 92.

dence they offered was complete and apparently free from all fallacy. From that time intermittent fever almost completely disappeared from among the convalescents in the Bahia Hospital.[1] Of course Dr. Dundas, who ignores malaria, attributes these results to a cause very different from the mere shutting out of a malarial atmosphere; but his fact is more useful than his explanation.

Varro, in his Treatise upon Agriculture, relates that his namesake Varro, a Roman general, who was in great danger of suffering, with a large fleet and army, from a malignant fever at Conyra, having discovered the course of the miasmata which produced it to be from the south, he fastened up all the southern windows and doors of the houses in which his troops were quartered, and opened new ones to the north, by which means he preserved them from the fever, which prevailed in all the other houses of the town and neighbourhood.

This practice, to which attention was specially called by Thouvenelle, in his clever, but eccentric work on the climate of Italy (iv. 13, 14), had not escaped the notice of Dr. Rush, who informs us that, in 1793, several families, who shut up their front and back doors and windows, escaped the disease (iii. 83). It is probable, indeed, let it be remarked *en passant*, that the benefit accruing from seclusion in times of severe epidemics of yellow fever and plague may be explained in this way, and not, as is supposed by contagionists, by the avoidance of individuals affected with the disease, or of objects supposed to be contaminated with the poison.

Let not the distance to which the cause of fever is here said to be occasionally wafted by the wind, deter the reader from lending a willing ear to this mode of transmission. Many facts are recorded to show that, through the same agency, other substances, diffused in the same manner in the atmosphere, as well as some evident to the sight, and of much heavier nature than the poison in question, have been conveyed to a considerable distance. We are told, on reliable authority, that on the 7th of May, 1842, soon after the outbreak of the great fire at Hamburg, an unusual and strong empyreumatic odour was experienced at Potsdam, at a distance of more than sixty leagues, and evidently proceeding from the direction of the burning city. The ashes of Vesuvius are sometimes wafted as far as Venice or even Greece, the distance of which from the volcano is not less

[1] Sketches of Brazil, 233.

than 500 and 700 *kilometres* (375 and 525 miles). Instances are mentioned of volcanic ashes having been conveyed much farther. We learn that vessels at sea have, while sailing at the distance of 700 to 1,000 kilometres (675 and 750 miles), west of the African coast, been covered with the red sand of that region.[1] In 1812, the ashes of the volcano of St. Vincent were conveyed by the wind as far as Barbadoes. On the same occasion those ashes fell to the depth of several inches on the deck of a vessel at the distance of 181 leagues East of St. Vincent. In 1815, the ashes of the Tamboro, in the Island of Sumbawa, were conveyed as far as Java, (108 leagues), in such large quantities as to obscure completely the atmosphere.[2] The odour of burnt turf, which characterizes the exhalations of the Westphalian marshes, has, we are told, been sensibly felt as far as Brussels, Liège, and even Paris.[3]

The effects of drying, overflowing and reclaiming marshy and sickly localities, and vice versâ, *prove the existence and agency of malaria.*— It may not be improper to bear in mind that the banks of water-courses, of marshy grounds, of ponds and lakes, as well as humid alluvial surfaces, become, during the drying process, the seat of febrile diseases of the periodic kind; while other localities in the neighbourhood, but where such a desiccation is not going on—the centre of lakes, or river streams, the open sea, beyond the influence of land air—remain healthy. Let those sickly surfaces be covered with water, either artificially or otherwise; cover masses of decomposing materials with earth; remove collections of putrid rubbish, decayed timber, or decomposed grass, or other vegetable matter, and fever, which before prevailed, will disappear, or greatly lessen. Dry up marshy surfaces, improve and reclaim the land by artificial means, and the same beneficial results will be obtained. On the other hand, let a surface of country heretofore healthy—whatever be its dimensions—be converted into a marsh, morass, or anything of analogous kind; as is yearly seen in various parts of Africa, Asia, and America; let a river bank be temporarily overflowed, and then partially dried; let a portion of low land daily covered by water at high tide, be imperfectly banked, in such a way that the admission of the water is only partially prevented, while its

[1] Becquerel, Sur les climats, 256. [2] Boudin, Géogr. Méd. 70.
[3] Dict. Pratique, art. Emanation, vii. 74.

complete egress is impeded; cut down a forest, and thereby expose a virgin soil, rich in organic matter, or a heretofore harmless, humid surface, to the sun's rays; upturn the earth, make deep excavations, dig canals, cut down bluffs, partially remove the water from, and thereby lay bare the bottom of, ponds; let the alluvion of a river encroach on the water and give rise to an extension of new-made and imperfectly dried ground, and fevers, other circumstances aiding, will certainly occur or become more rife. All this is placed beyond the reach of doubt. It is, of course, impossible, in the space to which I am necessarily limited, to illustrate these statements by more than a brief notice of the principal facts connected with the subject, which present themselves to my memory.

The first effects of clearing land, &c. injurious.—Dr. Rush remarks, in reference to the epidemics of Pennsylvania, that intermittents and mild remittents were converted, from clearing the country, into bilious and malignant remittents, and destructive epidemics; and that it was not until after years of cultivation that general salubrity followed. A like change has been found to occur in most parts of New England as well as in our Western and Southern States.[1] As the tide of emigration advances westward, remarks an intelligent writer, these (lake fevers), the prevailing fevers of Canada, retire before it. Kingston, situated at the eastern extremity of Lake Ontario, is, in 1840, very much healthier than it was in 1830; and Danville, two hundred miles, and Amherstburg, four hundred and thirty miles farther west, where, now, there are several cases of ague in every house in the course of the year, will, probably, in either ten or twenty years, be as healthy as Kingston is now. Besides there being less fever at Kingston than at the other two places mentioned, its type is intermittent, while at Danville and Amherstburg it is often remittent.[2]

Again, we find this occurring in middle Georgia, particularly in that portion of it lying west of the Oconee.[3] Here the country boasted, at the origin of the settlement, of a salubrious climate. Ten years after, when the forest trees had been levelled to the

[1] Everywhere, the first clearing has been attended with an increase in the number of cases, and in the malignancy of fever; and, in all, healthiness has followed after some years of settlement.

[2] Stratton, Brief Notes on the Lake Fever of Canada, Edinb, J. 55, 348.

[3] Pendleton, Charleston Med. J. vii. 451.

ground, the inhabitants began to suffer much from autumnal fevers, and continued to do so for the next twenty years. "Since it has become comparatively an old country, and few new grounds are brought into cultivation, it is quite healthy, the fevers being confined to the more marshy districts." See, as an example, what has been observed in the single town of Milledgeville. Dr. Fort, in a communication on bilious remittent fever, published in the *Southern Medical and Surgical Journal*,[1] informs us that this fever appeared there as suddenly as the face of Nature had been changed by the hand of man, and that for eighteen years, during the summer and fall, it was a formidable epidemic. "Forty years have elapsed since the settlement of that town, and the face of the surrounding country, in that time, has been completely changed. The rich soil has been washed away from the hills, and its deeply sanded bottoms have become dry. The sources of malaria have been dried up, and the mortality, which, in Milledgeville, at one time, from bilious fever, might have been five per cent. per annum, has diminished until now the deaths from this cause do not amount, annually, to more than one in two thousand."[2]

The late Dr. Williamson, of North Carolina, relates the following: "A gentleman in Craven County, lived on his farm above forty years without suffering by intermittent fevers, though his family consisted of fifty or sixty persons. There were about 100 acres of clear ground in front of his house that had been cultivated many years; but there was a thick wood behind the house. In the beginning of the year 1785, he caused all the timber and shrubs that were behind his house, within four or five hundred yards, to be cut down. His object was pasture and a free circulation of air. One-third of his family, on the next summer, was taken down by intermittent fevers. Such complaints were not more prevalent than usual during that summer, in other parts of the flat country. Those fevers were certainly caused by exposing to the sun a large surface of fresh land, covered with putrescent vegetables."[3]

Dr. Heustis, in his excellent work on the fevers of Alabama, alludes to a similar fact in the following remarks. "For the first three years after my arrival in this State, in 1821, 1822, and 1823, the country was dreadfully sickly, and the mortality great and ap-

[1] Nov. 1848, iv. No. 11.
[2] The same statement is made by Dr. Fort, in his Medical Practice, 67.
[3] Med. and Philos. Register, iii. 344. Hist. of North Carolina, ii. 193.

palling, more especially near the rivers. The whole country was then new, and the warmth and humidity of the season caused a great and rapid decomposition in the recently exposed and turned up vegetable matters. Many flourishing towns upon the rivers, which had risen up, as it were, by the hand of enchantment, received a sudden check, and became suddenly, almost totally, abandoned, from death and desertion. Strangers from every part of the United States, invited by the fertility of the soil, and the beauty of the country, and the serenity of the climate, brought together by fortuitous associations, with foreign and unseasoned constitutions, were suddenly swept off by thousands. In many families there were not well persons sufficient to attend upon the sick and dying. Never have I known a time of such general calamity."

Similar results will be found recorded in the medical histories of Pennsylvania, New York, and all our other States, as well as in those of England, France, Switzerland, and other sections of Europe, South America, and the West Indies.[1] In a word, everywhere we find a confirmation of the remark long made by our sagacious Franklin, "that all new countries are healthy for a certain period after their first development. That they become inhospitable to man, in a partial state of cultivation, and again healthful when the whole soil shall be necessarily cultivated to supply the wants of a population."

Partial draining injurious.—Examples of the injurious effects of partial draining and desiccation of wet localities, by artificial or natural means, and conversely of the beneficial results attending complete draining of marshy and insalubrious surfaces, or their complete submersion, are numerous and conclusive. They establish, beyond controversy, the fact that the insalubrity of marshy localities increases in compound ratio to the degree of dryness they have attained. They show that the greatest insalubrity and mortality in such localities always coincide with the period of the greatest desiccation, short of complete dryness; that this effect occurs earlier

[1] Volney, Climat des Etats Unis, 309; Forry, Climate of the U. S. 313; Drake, 249, 381, 8, 396, 404, 710, 717; U. Parsons, Dissert. 206; J. M. Smith on Epids. 73; Evans, 15–23; Williams, ii. 422; Macculloch, 126, 138, 172; Monfalcon, 180, 181; Cyclopedia, iii. 61-2-4-82; Ludlow, N. Y. Med. and Phys. J. ii. 83; Leblond, 23; Lefoulon, 23; Julia, 129; Boussingault, An. de Chimie, lvii. 151; Copland, ii. 758; Fenner's Rep. ii. 932; Transaction of Penns. State Med. Soc. ii. 42–65.

in hot than in cold latitudes, where the drying process is slower; earlier, when the season is precocious, and the reverse when it is tardy. In illustration of the injurious effect of imperfect drainage, by which a surface covered with water, and which before was comparatively innocuous, is converted into a pestiferous spot, the oft-mentioned case of the monastery of the Chartreuse, near Bordeaux, in France, may be cited. "A succession of bad fevers, before unknown, commenced immediately upon the removal of the water and the partial dryness of the land, showing themselves first in that part of the town which lay nearest to the land reformed, and lasting through several years."[1] In 1793 (4th Dec.), the revolutionary government of France, with that questionable sagacity which characterized most of its measures, ordered the drying up of every pond which was susceptible of the change—the reason assigned being the necessity of correcting the vitiated air supposed to be produced by the water therein contained. Nineteen months after, on the 1st of July, 1795, this law was repealed; because, in many localities, the drying process having converted ponds into marshes, which were partially desiccated after the rainy season, rendered the air much more insalubrious than it had ever been before. The report made on the subject to government by Creusê Latouche, judiciously recommended the reconversion of the marshes into ponds.[2] A similar accession of fever from partial and imperfect drainage occurred at Orville la Riviere, in France (Department of the lower Seine).

Injurious effects of the overflow of land.—The extensive prevalence of fever during hot weather, after the overflow of river, lake, or pond banks, and especially at the receding of the water, is well known to all medical readers, and has been noticed everywhere, and at all times.[3]

[1] Macculloch, 114.

[2] Villermé, Annales d'Hygiène, xi. 362; Parent du Châtelet, *ib.* xi. 308.,

[3] Drake, 130, 280, 372, 391; Bancroft, 294; Cycl. of Pract. Med. iii. 61; Evans, 25; Macculloch, 56–76–92; Williams, ii. 431; Mém. de l'Ac. de Méd. xiii. 644; xiv. 117, 119; Humboldt, 762; Schilizzi, cited by Melier, Mem. de l'Ac. de Med. xiii. 644; Am. Quart. Rev. iv. 294; Copland, ii. 758; Garrison Tr. Am. Med. Association, ii. 191; Leblond, 183; Nepple, 135; Jacquot, 11–22; Thevenot, 232; Second Rept. of Lond. Commissioners, 1848, p. 40; U. Parsons, 205; Ludlow, N. Y. J. ii. 86–7; Perier, Hyg. des Pays. Chaud. ii. 174; Fenner's Rept. i. 357, ii. 448; Barton Rept. 34; Warden, Nat. Hist. of Kinderhook, Med. Repos. ix. 619; White, Med. Repos. ix. 45, x. 36; Transactions of the Med. Soc. of the State of Pennsylv. ii. 71–133; Buel Webster's

At Massouah (Egypt), fever occurs every time the sea overflows.[1]

Those of our readers who have examined the writings of Lancisi, may recollect the epidemic of Balnesregium, a small town of Tuscany, an interesting account of which was sent to Lancisi by the Bishop of the place. The disease, on that occasion, was evidently due to the fall or caving in of a hill-side damming up a river stream, and thereby occasioning the inundation of the country around, and the exposure of the subjacent soil to the action of the sun's rays.[2] The epidemics of Pesaro, in 1708, and of Toronte and Trasilone,[3] cities of the Compagna, in 1709, also referred to by the same writer, were equally satisfactorily traced to a kindred cause. The inundations occasioned by the overflowing of the Tiber, and the disease resulting therefrom, are referred to by Livy, Dionysius of Halicarnassus, Dio Cassius, Strabo, &c. Like effects were observed and noted in the twelfth and thirteenth centuries, under the pontifical reign of Innocent III., and, in the fourteenth, under that of Clement V., and are particularly described by Lancisi, who accurately pointed out some of the causes of the disease to which they gave rise. Lancisi informs us that the river overflowed its bank in 1695. The water spread over a large expanse of country, filling the ditches, the sewers, and the canals. This, too, was followed in June, July, and August, by extremely hot weather. Decomposition of the submerged soil ensued, and this was followed by a malignant periodic fever, which spread far and wide, and occasioned a great mortality.[4]

The epidemic of pestilential, or, as we presume, malignant autumnal fever, which occurred along the Po at the commencement of the sixteenth century, and is described by Fracastorius, was occasioned by the overflow of that river, by which a large portion of the country was covered with water, and several extensive marshes were formed.[5] The epidemic of Ferrara in 1728, for an account of which we are indebted to Lanzoni, was evidently produced by the decomposition of organic matter arising from the large quantity of rain of the preceding years, followed by intense heat.[6]

collection, 54, 55; James Johnson, 43, 130, 360; Smelt, Med. Repos. ix. 125; Harrison, *ib.* x. 6; Hildreth, *ib.* xi. 346; Pitt, *ib.* xi. 337, Piltson, *ib.* v. 137; Worthington, *ib.* viii. 372; Lipscombe, Trans. Am. Med. Assoc. vi. 322.

[1] Auber Roche, An. d'Hygiène, xxxiii. 22.

[2] De Noxiis, &c. lib. ii. 210.

[3] *Ib.* pp. 245 and 334.

[4] *Ib.* 149, &c.

[5] Opera Omnia Philosophica et Medica, 1555, 4to. 100.

[6] Opera Omnia, iii. 4to. Lausanne.

The city of Strasburg, in France, is not often visited by malarial fevers. In 1824, the banks of the Rhine were overflowed, and remained for some time under water. Soon after the water had receded, fever began to prevail, and continued to do so during three consecutive years; nor did it cease before the soil had become perfectly dry. The occurrences which followed the great crevasses of 1816 and 1849, in New Orleans, and the inundation, from the Pontchartrain Lake, of a large portion of that city, in 1830, are fresh in our memories; as also those recorded in Italy, Germany, Egypt, India, Senegal, Algeria, and very many parts of our own country. The irrigations at Oran, Karguantal, Sibi-bel-Abbas, and other districts of Algeria,[1] where the practice is extensively applied to agricultural purposes, and is carried to such an extent as to occasion a sort of daily inundation; those of some of the departments of France, as well as those resorted to in the rice plantations of this country, have been found to give rise to the same morbific effects, wherever and whenever the thermometrical condition of the atmosphere is such as to aid in the extrication of malarial effluvia.

Mr. Lee, one of the inspectors employed by the London General Board of Health, in reporting on the irrigation by water-meadows in Wiltshire, thus states his information in relation to them. It not only shows, as an agricultural fact, the enormous quantity of water requisite to produce a high state of fertility on the open gutter and bed system of irrigation, but also that, for about half the whole year, these meadows are under water. The first "turn" is during winter, seven days on and seven days off. The second "turn" in spring, four days on and four days off. The third in summer, three days on and three days off; then three days on and six days off. In every "turn," except the last, the periods of irrigation are equal to those during which the water is shut off; but, during the frost, the water is kept on, if possible, altogether. These "turns" include night and day. The whole of these meadows, therefore, during about half the year, form one large evaporating surface, as much as would be the case if the whole were constituted an immense lake. Even when the water is off, the ground is so saturated that the evaporation must still be going on. No fen or morass in any low-lying, ill-drained district of the country would, I apprehend, impart, area for area, an equal amount of moisture to the atmosphere. The

[1] Jacquot, 23. Fièvres à Quinquina.

consequence is such as might be expected—ague is extensively prevalent in this valley, and is in some spots so general that scarcely any of the inhabitants, rich or poor, young or old, escape it.

The difference between the people exposed to the influence of such a surface and others living in neighbouring parishes of equal population, but not subject to malaria, may be seen, as Mr. Lee remarks, in the dejected and haggard appearance of the former, and in the greater amount dispensed among them for poor-rates. In the course of his investigation into the sanitary condition of the parish of Longbridge Deverill, near the town of Warminster, Mr. Lee found that in one portion of that parish, the large village of Crockerton, scarcely any of the inhabitants had escaped ague; and, on comparing the poor-rates for seven years back with those of the parishes of Corsley and Horningsham, of about equal size, and in the same union, it is seen that the charges for sickness in Longbridge Deverill are nearly double.[1]

The conclusions as to the insalubrity of common irrigation, and its unfitness for the proximity of towns, are fully corroborated by the fact that, in the Lombardo-Venitian provinces, where there is some of the oldest, most extensive, and skilfully conducted irrigation in Europe, the government has long found it necessary to interfere for the protection of the health of towns. By law, as stated in some information on the subject received by the English General Board of Health from the authorities of Milan, "permanent" irrigations are prohibited within five miles' distance of towns. These permanent irrigations are there mostly applied to the cultivation of rice. Cases are reported, from the irrigated districts, of the appearance and disappearance of fevers coincident with the operations of flooding and drying particular tracts of land. It appears, indeed, from extensive experience, that, wheresoever water is laid on the land in greater quantities than it can immediately or very soon absorb, or wheresoever there is alternate wetting (in such excess) and drying, malaria is apt to arise.[2]

In the Department of Marne and Loire, in France, the basin of the Anthion is 32 kilometres in length, and 6 in its greatest breadth, opposite to Corné, and contains 1,560 hectares, or 3,000 acres of *submersible* land between the bridge of Bourgignon and that of

[1] Minutes of Information, &c. on Sewer-water and Town-manure, &c. London, 1852, pp. 7, 8.

[2] *Ibid.* 9.

Forge, and 1,016 hectares, or 2,032 acres, equally *submersible* land, in another direction. It constitutes an immense focus of decomposition, from which are evolved effluvia, and often paludal miasmata. In the more depressed portions of this surface, the inundation is as much as ten feet deep. It is difficult, however, to obtain a complete desiccation of it. In 1843, the whole of this basin was flooded, and all the ditches completely filled. The Anthion, that year, remained higher than ordinarily. The water continued to cover the greater portion of the soil, and it became impossible to mow the grass in a dry state. This circumstance of an immense inundation ought, it would seem, to have proved advantageous; for the whole basin was, by the overflow, converted into an immense lake, by which an obstacle was put to the formation of a marsh. But no sooner had the water somewhat subsided, and, by so doing, allowed the exposure during the hot weather of July and August, of the banks of the ditches, and of a few hillocks, than the effluvia, then extricated from the mass of decomposing vegetable and animal substances therein contained, were wafted by the S. S. W. wind to the town, and infected the inhabitants, among whom it occasioned the development of an extensive epidemic of intermittent fever.[1]

The following statement is taken from a clever communication contained in an English periodical:—

"The summer in this country (Russia), is farther remarkable, inasmuch as, from the end of May to the beginning of September, no rain falls, and thunderstorms are extremely rare. The phenomenon is doubtless owing to the flatness of the country. For five hundred miles and more, around Perm and Kassan, there is not a hill of any consequence, and the whole tract from Kiew to Ural, for a breadth of five hundred miles, may be called a plain, only here and there interrupted by ranges of gentle hills. The extraordinary fertility, especially of the government of Kassan, is occasioned by the inundation of the Wolga, which overflows annually at particu-

[1] Gaultier de Claubry, Mém. de l'Acad. de Méd. xiv. 119, 120; see also 117, for another case of the same import.

The Canton of Amizy-le-Chateau is situate in a valley, on the south side of the city of Laon. In the centre runs the small, sinuous, and shallow river of Elete, which is supplied with water from a number of creeks. In the neighbourhood, there are a number of ponds, the whole subject to overflows, which, combined with the peculiar nature of the soil, have given rise to extensive marshes, well known under the name of the southern marshes of the Laonnais. The canton is very subject to fevers (Bulletin, i. 154).

lar seasons, as regularly as the Nile in Egypt, and converts the whole country, to the distance of ten miles or more from its bed, for five or six weeks, into an immense sea. These inundations of the Wolga, and the other large rivers, the Witjälka, the Kama, the Kinel, the Irgis, &c. which discharge themselves into the Wolga, render the countries through which they flow at once lively and fertile. At such seasons you may sail, either for pleasure or upon business, in large two-masted vessels, carrying from six to ten guns, over pastures and cornfields, to the neighbouring towns, which, on this account, are all situated upon heights; and, when the waters have withdrawn into their accustomed channels, the ground forsaken by them is covered often a yard deep with a fertilizing mud, in which, during the hot season, all vegetables grow rapidly and vigorously as in a hothouse. At the same time, pools are left behind in the low grounds, where the water stagnates for several months, becomes putrid, and generates putrid fevers in the months of July and August in these otherwise healthy countries. The government of Ufa, particularly, is visited about this time by an intermittent fever, which attacks the patient every seventh day only, but is so violent that it generally proves fatal."[1] "Near the walls of a large city," says a celebrated writer, "stood a very extensive and deep pond of water, which, for forty years, had served as a receptacle for all the filth from the houses and streets. As long as these putrid matters remained covered with water, they were productive of no mischief; but, when they had so far accumulated as to rise above the surface of the water, a most malignant fever spread through the tract of country adjoining the city."[2]

Dr. Cadwallader Colden, of New York, who wrote a full century ago, makes the following remarks:—

"It is well known that the Paltz River or Wallkill, in Ulster County, in this province, has been long taken notice of, as very prejudicial to the health of those who live on the banks of it. The waters of this river are of a dark colour, and come from a large space of ground overflowed with stagnating water. The inhabitants along this river, are yearly afflicted with intermittent fevers during the summer season, and a constant fog or vapour is observed almost all the summer (except in the time while the N. W. or northerly

[1] Notices of Russia, United Service Journal, January, 1833, p. 49.

[2] Senac, De Nat. Febr. Recond. lib. i. cap. vii. fol. 34, 35. Caldwell's Trans. 20.

winds blow) to arise over that river, and to remain there at a certain height and distance every morning, till the heat of the sun disperses it, and frequently, likewise, in the evening."[1]

About the same time a distinguished physician, of this city, stated the following fact: "A farm, within a few miles of this city, was remarkably healthy for fifty years, whilst the tide overflowed the low lands, near the dwelling-house; but after they were banked in by ditches, so ill-contrived that they did not often discharge the water that fell into them for a considerable time, it became putrid, and thereby rendered the place as remarkably sickly as it had been before healthy. I was told by a gentleman of veracity that he saw the corpses of nine tenants, that had been carried from it in a few years."[2]

Between Winchester and Charlottesville, in the valley of Virginia, is situate a remarkably large and deep spring, from which extends a low, marshy piece of ground about a mile long, and perhaps one hundred yards wide. All around this marsh the people have been annually subject to fevers in an unusual degree. In the very wet year of 1823, however, the marsh being inundated, they almost entirely escaped.[3] The readers of Pringle will remember the occurrences in Brabant during the campaign of 1748. "The country bordering upon the lower part of the Maas is not only unhealthful on this account (the humidity of the soil under the surface), but, by reason of floods from the small rivers, lies all the winter under water, and continues damp throughout the summer. The moisture and corruption of the air were much increased by the inundations (which had been made about the fortified towns since the commencement of the war), and sensibly became more noxious upon letting off part of the water, in the beginning of the summer."[4]

Dr. Robert Hamilton, of Lynn Regis, in a pamphlet quoted by Bancroft, and referred to particularly in the *Lond. Med. Gaz.*, describes a remittent fever produced in that place, in 1779, by a freshet which occasioned an inundation from the sea. "The inundations from the sea are generally followed by severer consequences in respect to health, than those from the fresh water. If they extend far, they cover much low ground under cultivation, and fill many ditches,

[1] N. Y. Med. and Philos. Register, i. 323.
[2] Bond, Introd. Lect. N. A. Med. and Surg. J. iv. 270.
[3] Cooke on Epid. Fevers, Med. Recorder, vii. 457, 8.
[4] Diseases of the Army, 61.

which, in many situations, cannot be drained by any other means than evaporation by the heat of the sun." "The remittent fevers which follow are of the worst kind;—the effect being due to the dead fish that remain, and the effluvia from the destruction of reptiles, insects, &c., and vegetables which are destroyed by the sea water." The gale of 1779 was attended by such an inundation, the effect of which, developed by the heat of five successive summers and autumns, were seen in the fevers of those years, which were more violent, universally epidemic, and more fatal than Dr. Hamilton had seen them in the past forty years. These fevers have ceased to show themselves. The country around, which was one of the most unhealthy, has become one of the most salubrious, by the complete draining of the Bedford level.[1]

"Near Guérande and Pont Chateau, several localities on the coast of Britanny partake more or less of the insalubrity of marshy surfaces. Among these may particularly be cited the towns of Dinan, Dol, Lamballe, and their environs, in consequence of the high tides occurring on this part of the coast, and resulting from the obstacles to the free flow of the sea from the western coast of the Cotentin. The waters thrown back by that coast are carried to, and accumulate in, the bays of St. Michel, St. Malo, and St. Brieux, where they rise to the height of forty feet, and overflow the country far beyond Dinan, spreading miasmata to the north and south." In the same way, in the Bay of Isigny, the sea rises to a great height, and occasions the overflow of the rivers, especially in the vicinity of Carentan, which is surrounded by stagnant water during several months of the year. "Hence the city and its environs are seldom free from fevers and other kindred diseases."[2] "In the province of Poitou, the insalubrity of most of the towns and villages arises in like manner from the exhalations generated in the morasses and ponds by which they are surrounded, owing to the overflow of the rivers. Thus the Sevre, in which the sea penetrates, overflows its stagnant waters during several months of the year, to the depth of two, four, and even six feet, and inundates the whole country around Luçon, Maillezais, and Marans, as far as two leagues below Niort, thereby forming a marsh of 65,085 arpents in extent. All this country is subject to intermittent fevers and other diseases."[3]

[1] Lond. Med. Gaz. xxviii. 790.
[2] Boncerf, Mém. de la Soc. Roy. de Méd viii. 282, 3.
[3] *Ib.* 282.

Mr. Ives, in the narrative of his travels from India to Europe by land, relates the following interesting and apposite fact:—

"After sailing up the River Tigris, from Bassora, we arrived at Bagdad. In this city, supposed to contain 500,000 souls, a purple fever then raged; but though it was computed that an eighth part of the inhabitants were ill, yet the distemper was far from being mortal. Here we were informed that the Arabs had broken down the banks of the river near Bassora, with a design to cover with water the deserts in its neighbourhood. This, it seems, is the usual method of revenge taken by the Arabs, for any injury done them by the Turks in Bassora; and it was represented to us as an act of the most shocking barbarity, since a general consuming sickness would undoubtedly be the consequence. This was the case fifteen years before, when the Arabs, by demolishing the banks of this river, laid the environs of Bassora under water. The stagnating and putrid water in the adjacent country, and the great quantity of dead and corrupted fish at that time lying upon the shore, polluted the whole atmosphere, and produced a putrid and mortal fever. Of this fever, between 12,000 and 14,000 of the inhabitants died; at the same time, not above two or three of the Europeans who were settled there escaped with life."[1]

In the summer of 1780, a violent remittent prevailed in that city, so general in its attacks, and so fatal in its effects, that it destroyed 25,000 persons, and was called a plague. The disease on this as on other occasions was manifestly owing to the cause mentioned, which, after moistening to saturation a great extent of the banks of the river, was speedily followed by intense solar desiccation, during which the fever appeared.[2] Indeed, the same effects are frequently produced at Bassora, and to a highly destructive degree, after the ordinary overflowing of the Euphrates. Of the consequences arising from simple inundation, Egypt affords, as we have seen, a similar example; inasmuch as its season of fever commences with the subsidence of the Nile. Every one must know, that equally disastrous results have often followed the overflowing of the Adige, the Po, the Scheld, the Saave, the Theisse, the Don, the Tigris, and other streams.

Hungary is as sickly a country as any other in Europe, and is

[1] Lind, on Hot Climates, 118, 119.

[2] Craigie's Practice, i. 172; Trans. of a Society for the Improvement of Med.-Chir. Knowledge, ii. 55.

scarcely exceeded in that respect by any on this side of the Atlantic, or in Africa. Its fevers, which in the short space of a few years destroyed forty thousand Austrian soldiers, has, under the name of *morbus Hungaricus*, become familiar to all medical readers. Here, the same causes which render other localities injurious to health, and prolific of febrile complaints, subsist in an eminent degree. "Hungary abounds in rivers, some of great magnitude—the Danube and the Drave, which, by often overflowing, leaves that low, flat country overspread with lakes and ponds of stagnating water, and with large unwholesome marshes."[1]

The fevers of Senegal, described by Thevenot, and before him by Schotte, are due to the annual inundation of the river of that name. Containing but little water during eight months of the year, it fills up rapidly during the wet season, and, soon rising to a height of more than 38 or 40 feet beyond its proper level, spreads—as do also its many tributary streams and neighbouring lakes, which before were dried up—its waters over the whole country, which now presents, like Egypt, during the inundation of the Nile, the appearance of a vast expanse of water dotted over with villages. On the subsidence of this, fever breaks out.[2]

"The garrison of Fort Augusta, which stands very near some marshes, to which it is to leeward when the land wind blows, was yet remarkably healthy; but it became at one time extremely sickly upon the breaking in of the sea in consequence of a high tide, whereby the water, which was retained in the hollows of the fort, produced a putrid moisture in the soil, exhaling a vapour offensive to the smell, and with all the noxious effects upon health commonly arising from the effluvia of marshes."[3]

The yellow fever epidemic of La Guayra, in 1797—the first known to have occurred in that place—has been referred, with much plausibility, to the overflowing of the river of that name. "This torrent, which in general is not ten inches deep, was swelled, after sixty hours of rain in the mountains, in so extraordinary a manner that it bore down trunks of trees and masses of rocks of a considerable size. During this augmentation, the waters were from thirty to forty feet in breadth, and from eight to ten feet in depth. Many houses were carried away by the torrent, and the in-

[1] Lind on Seamen, 59; Pringl. 189.
[2] Thevenot, Maladies des Européens, &c. 20, 21.
[3] Blane, Diseases of Seamen, 2d edit. 229, note.

undations became more dangerous for the stores, in consequence of the gate of the town, which could alone have given an issue to the waters, being accidentally shut. It was necessary to make a breach in the wall in the sea-side; more than thirty persons perished."[1]

Bad effects of copious rains followed by great and desiccating heat.—A wet season, or heavy temporary rains, by converting dry and heretofore healthy tracts of country into swamps or marshes, or by merely thoroughly softening a soil previously hardened by a dry spell of weather, and mostly destitute of vegetation, has, when followed by heat, given rise to a considerable extent of disease.[2] Hence, in localities which are naturally dry, or which have been rendered so by long-continued and intense heat, the rainy season is that of the greatest insalubrity, and continues so until the soil becomes completely submerged. Indeed, the fresh rains often become the signal for the breaking out of the disease. Among the many facts that might be cited in corroboration of the former statement, I shall select the following: In Corsica, fevers break out in August and September, not only along the marshes, but in the villages situate between the mountains, on the occurrence of heavy showers.[3] Speaking of 1839, Mr. Gouraud says: "I was beginning to think that the extent of the annual endemic had been greatly exaggerated. On the 20th of August, during a very hot spell of weather, we had a shower. The latter produced, instantaneously, a very offensive odour, and decided the development of pernicious intermittents."[4] Similar effects of the first rains are noticed in the Morea. On their landing there, in 1828, the French troops were exposed to intense heat by day and cold by night. These vicissitudes, succeeding to a long period of dryness, invited the approach of the annual epidemic. Rain fell in heavy showers on the 18th and 19th of September, and the disease broke out on the 20th.[5] A gentleman, living on the ridge land between the Opequon and the Shenandoah Rivers, in Virginia, informed Dr. Cooke, from whose essay on Epidemic Fevers I quote: "That he had, for nineteen years before 1823, scarcely known what sickness in his family was. A neighbour had been in the habit, for many years, of watering his meadow by small rills, from his mill-race. Before this wet year, the quantity he could spare was

[1] Humboldt, Pers. Nar. iii. 292.
[2] Villermé, An. d'Hyg. xi. 350; Leblond, 100, 184.
[3] Gouraud, Etudes sur les F. Interm. Pernicieuses dans les contrées méridionales, 37.
[4] *Ib.* 40. [5] *Ib.* 218.

very small, and not enough to make the ground wet, the moisture being absorbed immediately. This year, however, it was quite marshy, and the road through it very bad. The consequence was, that a great number of persons died in the houses just around the meadows."[1]

In 1830, the city of Macacu, in the Province of Rio Janeiro (Brazil), on the banks of the river of that name, suffered extensively from periodic fever. The district around the city is rich in virgin forests and in coffee, sugar, rice, and other plantations. Thence the disease extended far and wide, occupying a surface of argillaceous soil, cut up by numerous streams, torrents, and lakes bordered with mangroves, and subject to inundation at high tides. The epidemic was evidently due to a great drought, which occurred in 1829–30, and succeeded to heavy rains, which flooded the country.[2]

It may be mentioned, in illustration of this, that sickly seasons are generally those in which a wet spring is followed by a hot summer. We have seen that in Africa, in the East Indies, in the Antilles, and in South America, fevers make their appearance at the commencement and a short time after the close of the rainy season; when, consequently, the soil is not yet deluged with water, or when it has ceased to be so, and offers, by its exposure in a moist state to the evaporating power of the sun, and by the greater luxuriance of vegetation which ensues, ample food for the generation of the febrile poison. This is equally true, both as regards common autumnal, and malignant yellow fever. While they all often commence and rage soon after the appearance of rain, and are sometimes put a stop to by excessive and long-continued drought, they seldom break out in an epidemic form during a very dry spell of weather, unless the latter has been preceded by a wet spring, or a quick succession of heavy showers, by which the earth is thoroughly saturated. Facts illustrative of these effects have been observed everywhere, both abroad and in this country.[3] It is on those observed by him-

[1] Med. Recorder, vii. 457.

[2] Sigaud, du Climat et des Mal. du Brazil, 170–2.

[3] Town, 7–9; Warren, 8; Chalmers, i. 19–22; Macartney, quoted by Dickson, Edinb. Journ. xiii. 47; Humboldt, 765: Desportes, i. 17, 121; Lind, 50; Bailly, Fièvres Interm. 130; Clark, Med. Notes on Climate of Italy, 80; Tommasini, ii. 488, 9; Davidson, quoted by Rush, iv. 155; Halphen (of N. O.), 42, 43; Pallas, 209, 210; Report of Sickness of British Army, 4–23; Savaresi, 256, 257, 292; Pugnet, 344, 345; Mosely, 10, 11; Chisholm, i. 146, 147; Belcher, Ed. Journ. xxiii. 248; Daristo, 33; Davidson, Med. Repos. viii. 248, 249; Catel, 7; Dazille, 10; Ed. Journ. lxiii. 448; Dupré, Am. Journ. N. S. iii. 265; E. H. Smith, Webster's Collection, 75; Bayley,

self and others in the West Indies, though especially in Holland, Spain, and Portugal, that Dr. Ferguson built his theory of the exclusive agency of the *drying process*, that grew with him into a hobby, which he rode manfully and with much ability to the last day of his life, and which a few of our countrymen would seem disposed to mount, strangely unmindful of many other facts which strongly militate against its exclusive adoption, and show, as we have seen, that, under certain circumstances, malarial fevers not unfrequently appear under the influence of the *wetting process*.

The establishment of mill-dams, and the subsequent partial desiccation of the soil, have the same effect. Dr. O. W. Holmes, who, besides writing amusing poetry, composes excellent medical essays, adduces, in a work of marked ability, a large body of facts, showing the injurious effects of these dams, and of ponds.[1] Dr. Rush, in enumerating the causes of the increase of remittent bilious fever in and about the year 1785, mentions, prominently, the establishment of dams;[2] and Volney has expressed himself strongly in their condemnation.[3] Harrisburg, in this State, situate between the River Susquehanna and a small creek, and extending nearly or quite from one to the other, furnishes an acceptable illustration of the above fact. In the year 1773, a dam was built across the creek, in the rear of the town, and thereby produced a very extensive and shallow pond. In the fall of the year succeeding, a mortal fever prevailed in the town, which was satisfactorily traced to the influence of that pond. The people insisted on having the dam removed. The pond was completely drained, and the town was as healthy afterwards as it had been before that time.[4] Mifflin County, in this State, is generally free from sources of miasmal exhalations. In 1823, intermittent fever prevailed extensively; but it was confined exclusively to the immediate vicinity of mill-dams. A few cases, in the more healthy districts, could be traced, in every instance, to the same source, or to a visit to the river.[5]

53, 123; Ralph, Edinb. Med.-Chir. Trans. 55-60; Doughty, 187, 188; Lewis, N. O. Journ. for July, 1848, p. 38; Pinkard, ii. 60; Leblond, vii. 100, 106; Villermé, An. d'Hyg. xi. 350.

[1] Boylston Prize Dissertations (Interm. Fever) 47.

[2] Rush, An Inquiry into the Causes of the Increase of Remittent and Intermittent Fevers in Pennsylvania. Philosoph. Trans. ii. 206.

[3] Volney, Climat des E. U. ii. 309, &c.

[4] Cooke on Epid. Fev. Med. Recorder, vii. 452.

[5] Trans. of State Med. Society, ii. 44; see also p. 47.

Dr. Drake, in his imperishable volume, relates the following strong fact: The village of Washington, in the State of Ohio, stands on the north-east side of Paint Creek. About the year 1826, a mill-dam was erected a short distance above the town, which caused the inundation, to the depth of a few feet, of about sixty acres of bottom land. As the stream generally fell too low, by the first of June, to admit of grinding at the mill, it was the custom of the proprietors to open the floodgates, and let the water escape,[1] after which the copious showers of that month commonly washed away the recent deposits, and thus the health of the village did not appear to suffer. In the year 1838, the owners did not let off the water until July, and no rains followed to wash away the silt. In a short time an offensive smell was wafted from this foul and drying surface into the village, which was to its leeward, and, in the month of August, the inhabitants began to sicken with remittent and intermittent fevers. Those who lived on streets nearest to the pond, suffered most. The people who resided in the vicinity, to the west, or windward, did not suffer. No epidemic, so severe, had ever visited the village before. In the succeeding years, up to the time of Dr. D.'s visit, in 1840, the waters had been drained off the first of

[1] Speaking of the upper Cuyahoga Basin, Dr. Drake remarks that it is annually more or less infested with autumnal fever, although elevated eleven hundred feet above the sea, and in the mean latitude of 41° 30′ N.; but the banks of the ponds and marshes are much infested. "Thus, I was told by Mr. Coles, of Chardon, that when a dam was built across the Cuyahoga, at Burton, the people were, in the two next years, generally attacked by autumnal fever; and Dr. Hamilton, of the same town, informed me that, in the neighbourhood of Burton, a dam was demolished in summer or autumn, and nearly all the labourers engaged in the work sickened with fever. From Dr. Bennett, of Sharlesville, I received the following facts: The Cuyahoga flows near the western side of that village, from east to south-west. To obtain water for the Mahoning Canal, a dam was thrown across the river, which gave rise to many cases of fever. Two years afterwards a higher dam was erected, lower down the river, which raised the water to the level of the first, involved the ruin of an old mill, produced stagnant water in the mouths of many small streams, inundated some forest land, and so intercepted tho volume of water flowing in the river, that when it became reduced, in autumn, tho whole was transmitted through tho canal-feeder, leaving but a series of pools in the partially dried up river bed below. During the first autumn after this signal change in the condition of the river, no injury to health was experienced; but in the next, nearly all the inhabitants, on both sides of the river, above and below the new dam, were attacked with autumnal fever. Tho number of cases was estimated at one hundred and fifty. They who lived near the river had intermittents; those who resided farther off, and on higher ground, suffered more from remittents." Pp. 372, 3.

June, and much of the drift-wood and filth cleared away; in consequence of which, apparently, the epidemic had not occurred.

The same writer relates the following example: "On Cedar Creek, a tributary of the Cumberland River, a mill-dam had been erected, about sixteen feet high. After twenty-two years, the basin having become filled with silt and dross, the dam was torn down, and the perpendicular face of the deposit exposed to the action of the sun and air, in the month of August. The consequence of this was, that nearly all the men who performed this labour were seized with severe autumnal fever, and one died.[1]"

The following fact is related by another American writer, Dr. Ludlow, of New York. A mill-pond in the vicinity of Lyons, Ontario County, which overflowed ten or fifteen acres of land, was drained, in the summer of 1822, for the first time, the mill having been built about five years. In consequence, about thirty people in the immediate vicinity were taken suddenly ill, seven or eight of whom died. Some of the intermittents were very malignant.[2]

Macculloch refers to equally strong facts as observed in the iron district of Glamorganshire, at Hirwren, and other parts of Southend, near Lewisham, in Hertfordshire, and elsewhere in England.[3]

Injurious effects of upturning the earth in hot weather.—It has already been stated that the digging of canals, the opening of ditches, the cutting down of bluffs, the levelling of lots, the filling up or digging down of streets, the opening of roads, the establishment of brickyards, &c. have proved highly injurious, not only in this country, but in parallel or hotter latitudes, as also during the hot seasons of cold regions. The results of operations of the kind in the East and West Indies are on record, and, like those in Algeria, an account of which has recently appeared, are of a nature not to be easily forgotten. Near Tlemcen, in Algeria, nearly all the soldiers employed in digging a well were attacked with fever, while all the others in the vicinity escaped.[4] The opening of roads, and the upturning of the earth for various purposes, are recognized, by the entire body of physicians attached to the French army in that

[1] Topogr. &c. of the Miss. Valley, 717.

[2] A Stat. and Med. Account of the Genesee Country, N. Y. New York Med. and Phys. J. ii. 89.

[3] *Op. cit.* 101–103.

[4] Jacquot, Fièvres à Quinquina, 29.

country, as having given rise to the extensive development of fever.[1] The writings of Dr. Drake,[2] so often cited, those of Evans,[3] Blanc,[4] Cassan,[5] Macculloch,[6] Usher Parsons,[7] Caldwell,[8] Bayley,[9] Thomas,[10] Anderbach,[11] Davy,[12] Gaultier de Claubry,[13] and many others, abound in similar cases. Dr. Merrill, in an essay on the yellow fever of Natchez, has taken great pains to show, and with every appearance of success, that the epidemic of 1823 arose from the levelling of the streets, and the consequent exposure of the fresh soil to the action of the hot sun.[14]

The same able physician, in an excellent and interesting anniversary address read before the Medical Society of Memphis, Tennessee, on "the health and mortality" of that growing city, has traced the great sickness and loss of life that recently occurred there, principally to the work of grading and digging, which has been extensively carried on for several years past, and which produce an obstruction to free drainage. "In this," says Dr. Merrill, "I am sustained by the opinion of Dr. Grant, in his able address before this Society at our last anniversary, by many of the members of the Society, by the Board of Health, and by many persons of learning and experience who are not members of the medical profession."[15]

The injurious effects of these works is farther illustrated by occurrences in Charleston, in 1842 and 1852: "In 1842, white labourers, strongly predisposed to yellow fever, were employed in opening drains and other works, and transferring the earth to different portions of the city; and where the drains were opened and the earth was deposited, there yellow fever occurred, and the unfortunate beings who performed that work were the greatest victims. In 1852, the same thing occurred."[16] Dr. Simons remarks, in a note, that, in reference to the last-mentioned year, at the new custom-

[1] *Ibid.* 13-29.
[2] *Op. cit.* 182, 229, 235-239, 372.
[3] *Op. cit.* 75, 266.
[4] Dissertations, i. 332.
[5] Mém. de la Soc. Méd. d'Emulation, v. 142.
[6] *Op. cit.* 83, &c.
[7] Essay on Malaria, in vol. of Essays, 206.
[8] Essay on Miasm, Boston J. ii. 504.
[9] Yellow Fev. of New York in 1795.
[10] Essai sur la Fièvre J., 1st ed. 71.
[11] N. Y. J. ii. 75, N. S.
[12] Second Rept. on Quarantine (Lond.), 57.
[13] Mém. de l'Acad. de Méd. xiv. 120.
[14] Philad. Med. and Phys. J. ix. 340.
[15] Memphis Med. Recorder, i. 85-94; see, also, Grant, "The Meteorology, Sanitary Condition, Prevailing Diseases, and Mortuary Statistics of Memphis," American J. July 1853, 74, 115; see, also, a paper by the same, in N. O. J. for May, 1852.
[16] Simons, Charleston J. viii. 364.

house, a great number of Irishmen were employed in excavating the earth, and piling; that of these a great many were taken sick and died; and that the sale and distribution of the earth through different portions of the city had a baneful effect.[1] The same correct observer states, in addition, that "in 1849 an extensive drain was opened in Hasel Street, excavating the most filthy and offensive materials; and, likewise, an extensive drain in Market Street, from Church Street to the wharf. Yellow fever occurred earliest in those localities, and was more fatal. Again, the earth, so filthy and offensive, was transferred to King's Street, from Horlbeck's Alley to Hasel Street, and, in this particular spot, there was sickness among a class of persons who are generally exempt."[2]

For the following account of an extensive diffusion of autumnal fever from a kindred cause, which occurred in this country, we are indebted to an English writer, who collected the information on the spot.

The digging of that part of the Chesapeake and Ohio Canal which runs from Seneka to Georgetown, commenced on the 17th of October, 1828, and was nearly finished in November, 1830. The distance is about twenty-three miles, and for the greater part the canal runs nearly parallel, and very close to the Potomac River. The bank of the canal is elevated considerably higher, and, in some places, as much as fifty-four feet above the banks of the stream, which is there inclosed in a hilly country, and the water passes on to the ocean, with all the rapidity of a mountain torrent. During the summer of 1829, there were nearly four thousand labourers employed in making this division of the canal. They continued healthy until the end of June; but in July, August, and September, they suffered severely. During these months, two out of three were attacked with autumnal fever, most of them with its most aggravated form, and, as nearly as could be ascertained, of those that were taken ill, about one-fifth fell victims to the disease.[3]

All this is confirmed by what Lind pointed out, long ago—that "the effluvia from ground newly opened, whether from graves or ditches, are far more dangerous than those from the same swampy soil, where the surface is undisturbed; nay, in some places, it has been found to be almost certain death for an European to dig a grave in

[1] *Ibid.* 365. [2] *Ib.* 364, 5.
[3] Stevens on the Blood, 239.

swampy districts of tropical climates, unless long seasoned to the country. In such a place, the attendance of friends at funerals ought to be dispensed with."[1]

The construction of the canal of the Ourcq, in France, in the years 1810-13, will long be remembered for the extensive prevalence of fever, of which it was evidently the cause, and the large mortality which ensued at Pantin, and several other neighbouring villages.[2] We all have a recollection of the results attending the digging of the Carondelet, and other canals in or near New Orleans, and in other parts of the country.[3] A memorable instance of this kind occurred in France in the latter part of the seventeenth century, on the occasion of the construction of the aqueducts and canals required for conveying water to Versailles from the River Eure. The immense upturning of earth, which was effected during very hot weather, was followed by a large amount of disease and mortality all along the route, but more particularly about the Castle of Maintenon, where the principal works were constructed.[4]

Another instance of a more recent date, is worth recording. In the small commune of Lutzenbourg, in the arrondissement of Sarrebourg, in France, the population of which did not exceed six hundred and twenty-five, remittent fever, preceded by a vast amount of ague, prevailed during July, August, and September, 1852. The spring had been cold and wet; but about the first week of May, the thermometer suddenly began to rise. The heat steadily increased, and became intense in July. The disease, as well as the intermittents which preceded, were satisfactorily traced to the influence of the excavations and upturnings of earth required in the construction of the Paris and Strasburg Railroad, which passes through the commune, and in cutting the canal connecting the Marne with the Rhine, the bed of which, from the stoppage of the work, had been converted into a boggy marsh. "In proportion," says the reporter, "as the public works extended through (Sillonnaient) the valley, and by upturning the earth, brought up to the surface the deeper layers of the soil, so intermittent fevers arose and spread very extensively." Prior to this, the population had been healthy, and strangers to malarial fevers."[5]

[1] Dis. of Hot Climates, 168. [2] Villermé, An. D'Hyg. xi. 352.

[3] Barton's Rep. to State Med. Soc. of Louisiana, 34. Thomas, 70, 1st ed.

[4] La Beaumelle, Mém. de Mad. de Maintenon, iii. 239, 240.

[5] Burchardt, Gaz. Méd. de Strasburg, December 25, 1852, xii. 407. The esta-

I shall have occasion, as we proceed, to mention an event strikingly illustrative of the injurious effects of exhalation from open ditches, which occurred at the General Lying-in Hospital in York Road, Lambeth.

The following case, which is derived from Lancisi,[1] will be read with interest: On the first of May, 1707, the hilly grounds to the southward of the town of Bagnarea, after continued great rains, began to fall into the river which was near that town, in such a manner that whole vineyards were moved from their places, and some houses entirely, without falling; in one of them, a woman was delivered of a child while the house was on its march. The channel of the river was choked and filled up. Many cracks, gaps, and holes were left in several parts of the ground, in which the waters stagnated; and they, being impregnated with sulphureous minerals, with which the earth there abounded, became exceedingly offensive to the smell. During the summer heats, the colour of the inhabitants became of a dead swarthy yellow, and grievous pestilential fevers seized them. These were confined to the southern and lower parts of the town; while the other parts, which stood high, and at a distance from the stagnating waters, out of reach of the vapour which arose from thence, remained healthy, as usual. By order of the magistrates, the channel of the river was

blishment of the railroad from Strasburg to Basle, has required the digging, to the depth of 1 to 2 metres (from 3½ to 7 feet), of the adjacent fields, with the view of procuring earth with which to level the road. The results have been immense excavations in the vicinity of the communes of Bottwiler and of Feldkirch. After being filled with water in the autumn and spring, these excavations become partially dried, and deposit a large quantity of slime. They have thus been converted into true marshes, in which M. A. Boumann has discovered the characteristic plants of stagnant water.

Under the influence of this marshy surface the commune of Bottwiler, the population of which is 1446, has severely suffered from intermittent fever during the last three years. In 1842, the number of fever cases amounted to 36; in 1844, to 166; in 1845, to 743; and in 1846, to 1166. The mortality has increased in the same proportion. The average in ten years (from 1836 to 1845) is 36. In 1846, it was not less than 54. The small commune of Feldkirch, with a population of 450, suffered with equal severity. In 1843, the number of cases amounted to 2; in 1844, to 20; in 1845, to 135; and in 1846, to 376. (Dollfus-Aussat, Comptes Rendus de l'Acad. des Sciences, 1847, xxiv. 779.) M. De Gasparin stated, at the meeting of the Academy of Sciences, when this report was read, that similar effects were produced by analogous changes along the railroad of Tarascon. He added that, when informed of these facts, the Minister of the Interior ordered the marshes to be filled up. Archives, 4th series, xiv. 241.

[1] Nox. Palud. Effl. lib. 2, Epidem. iii. 210, 211.

cleared, drains were made for carrying off the water, the places where it stagnated were cleared, and the cavities, which could not be drained, were filled up, and the inhabitants were the next summer freed from the fevers.

In the public thanksgiving ordered for this deliverance, the Bishop declared the obligations they were under to Lancisi, by whose advice they had been delivered from such poisonous diseases.

The injurious effects resulting from the receding of the sea, and the consequent extension of the land by the deposition of alluvion, have been noticed at the mouth of the Tiber, of the Po, of the Ganges, of the Oronoco, of the Mississippi, &c. The partial revolution which has taken place in the first of these localities, as is remarked by Dr. Carrière, has not resulted merely in pushing back inland the old city of Ostia, formerly the seaport of Rome, and separating from the sea the original mouth of the Tiber; it has placed the city in the centre of an insalubrious territory, of which it formerly occupied only the edge, and has been the cause of its progressive depopulation. We know that formerly Ostia was too small for the number of its inhabitants, and figured among the magnificent cities of Latium. At present, it contains only a priest, a tavern-keeper, four or five soldiers, and a few families, who do not venture even to reside there all the year round. But this insalubrity is not limited to the city; for, inasmuch as the miasmatic elaboration is effected on a large scale, in consequence of the increased extent of surface, there has arisen, since the time of the Romans, a new source of morbific effluvia, which must be taken into account when we inquire into the sanatory conditions of the country, though more especially when the wind blows from the sea.[1] Another case in point is mentioned by Sir W. Burnett. In former days, there existed a large marsh near Mahon. It has now been converted into productive gardens. Since the draining of the greater part of this marsh has been completed, the sea has been gradually receding from the head of the harbour, leaving (particularly during the prevalence of the easterly winds) a large portion uncovered, from which, in the summer and autumn, the most offensive exhalations proceed.[2] I have already alluded to the effect of the annual receding of the Nile. The same effect results from the diminished extent, noticed every year at particular periods, in certain other streams and bodies of water, by which a

[1] Amer. J. July, 1851. Carrière, le Climat de l'Italie, 20, 21.

[2] Fevers of the Mediterranean, p. 14.

large portion of their banks is laid bare. The River Biviere, and other watercourses in Sicily, lose, yearly, two-thirds or more of their dimensions; Lake Cagliari, in Sardinia, loses almost as much. All are hotbeds of fever.

Beneficial effects of complete drainage.—On the other hand, while every where experience has demonstrated the injurious effects arising from the ordinary mode of cultivating rice—effects which have called forth in several places the protective interference of governments[1] —the physicians of Georgia are ready to tell us what has been the result of the dry-culture system on the health of localities heretofore proverbial for their sickliness.[2] In New England, and other parts of this country, the removal of mill-dams has, as in other instances already mentioned, been found to produce the like salutary effects. Several of the cities of England and France formerly contained extensive portions of marshy ground, and other sources of miasmal exhalations, and were, in consequence, annually visited by remittent and intermittent fevers. Many of these marshes have been completely drained, and the surface built upon, or otherwise improved, and the disease has disappeared. Thus, for example, in 1558, the mortality from such fevers in London was so considerable, that the living could hardly bury the dead. Less than a couple of centuries ago, that city was subject to the same disease, in its worst forms. Bishop Burnet's authority is cited for the fact, that, in the reign of Mary, intermittent fevers raged like a plague.[3] The same may be found in the writings of Sydenham and Morton. From 1661 to 1665, the annual loss from intermittents alone averaged more than one thousand. Dr. Short informs us that among forty deaths from fever, between 1628 and 1636, one was from ague. "There are some diseases on the decline," he says. "Agues, whereof one of forty of the whole that died of fevers, died; now scarce one of 1,100 that die of fevers, die of this. This distemper has at several times prevailed for a long series of years, and has sometimes raged like a plague. In 1664 they disappeared, and scarce came on the stage before '78; but from 1720 to 1729, they and remittents afflicted the

[1] Monfalcon, 160; Bourely, Gaz. Méd. de Montpellier, Oct. 1849, 99; *ib.* Annales d'Hyg. xliii. 328, 332; Cycl. of Pract. Med. iii. 60; Williams on Morbid Poisons, ii. 431; Boileau Castelnau, Annales d'Hyg. xliii. 331; Delongchamp, Dict. des Sc. Méd. xlix. 56, 7; Zimmerman, de l'Expérience, ii. 402; Fodéré, Méd. Légale, v. 153.

[2] Daniel, 29.

[3] Blane, i. 254, 255.

whole nation grievously; and now, as to their severity, especially mortality, they are extinct."[1] Remittents and intermittents constituted, as it would appear, no inconsiderable an item among the diseases which Fothergill noticed in London during certain seasons in 1751–4.[2] The immense marshes which were situate around the city, at Lambeth and other places, were removed by under-drains and sewers; ditches were filled up; the river, which, from Lambeth to Woolwich, was swampy, was banked out, and the site rendered dry by being covered with buildings, and the most satisfactory changes in regard to salubrity were obtained. In 1728, the deaths from these fevers amounted to forty-four; in 1729, to forty-seven; and in 1730, to only sixteen. In 1752, the proportion was one in one thousand of deaths. For some sixty years, the disease has not been known as an endemic in that metropolis; and of the few cases that occur, the larger number, if not all, are traceable to fenny districts, situate at a greater or less distance.[3] A similar improvement has taken place at Portsmouth, which, in former days, was a hotbed of intermittent fever; but has been freed of the disease, simply by paving the streets, and removing various sources of miasmal infection.[4] In England, according to recent reports, of the fifty cities and towns in which the mortality was highest, there was scarcely one in which drainage or sewerage was complete. In seven, it was indifferent, and in forty-two it was decidedly bad. Experience there has shown that no population is healthy which live amid cesspools, or upon a soil per-

[1] New Observ. Natural, Moral, Civil, Political, and Medical, on City, Town, and Country Bills of Mortality, &c. 208. Lond. 1750.

[2] Weather and Diseases of London.—Works i. 151, 210, 234.

[3] Dr. Good, and his commentator, Mr. Cooper, have called attention to the fact that, in 1822 and 1823, agues again made their appearance, and have prevailed ever since more frequently than they had done for many years before, adducing this as a proof of the disease arising often from other causes than malaria. These cases, however, have prevailed principally on the outskirts and not the centre of the great metropolis. Besides, as respects London and its vicinity, changes have taken place sufficient to explain the circumstance without negativing the existence of malarial effluvia. Dr. Copland, who is of that opinion, calls attention to the fact that "the streets have been macadamized, constantly watered, and covered by a wet, clayey mud; the soil surrounding the metropolis has been turned up for the purpose of building, &c., to a much greater extent since that period than formerly; and the muddy and marshy banks of the river have been unusually disturbed and inundated by the swell from the paddles of the numerous steam vessels."—Copland i. 1090, Am. ed.

[4] See British & For. Med. R. i. 286. See, also, Baker on Intermittents, Med. Trans. of College of Phy. of London, iii. 141, &c.; Willan's Reports, 203.

meated by decomposing animal or vegetable refuse, giving off impurities to the air in their houses, and in the streets; and experience has proved, also, that in those places where such sources of impurity are removed, the salubrity improves. From this, we may infer that a late writer is not far from the truth, when he avers that intermittents have nowadays become, to the profession at large, materials for medical history, rather than for medical observation; since it is a rare fact, in many places, to meet with a case. If they appear at all, it is in marshy districts, some of which exist still, and must long continue to do so, or in such places as along the Surrey bank of the river—from Battersea to Deptford, where a great part of the surface is lower than high-water mark, and the ground consequently is never well drained.[1]

In the Island of Ely (England), the mortality, as compared with the births, was formerly as 70 to 61. The island has been drained; and we learn from Sir John Sinclair, that the proportion is now as 54 to 61. Dr. Kirkland, who practised for thirty years at Chelmford, situate in a marshy district of country, called the Hundred of Essex, states that, in former days, the inhabitants were all pale, jaundiced, emaciated, and bore in every way strong marks of the effects of malaria. Fever prevailed extensively among them, and hardly any stranger could venture to the place. Through the instrumentality of philanthropic and enterprising individuals, the country has been drained, improved, and cultivated; fevers have disappeared or greatly lessened, and the inhabitants are as remarkable for their fine health and fine looks, as they were before for the reverse.[2]

Dr. Rigby, in his examination before the commissioners on the

[1] London Medical Gazette, xxiii. 876.

[2] Sinclair, The Code of Health and Longevity, 41, 42. London, 1829.

Mr. John Marshall, Jr., in a report, says: It has been shown that the Isle of Ely was at one period in a desolate state, being frequently inundated by the upland waters, and destitute of adequate means of drainage; the lower parts became a wilderness of stagnant pools, the exhalations from which loaded the air with pestiferous vapours and fogs; now, by the improvements which have from time to time been made, and particularly within the last fifty years, an alteration has been made which may appear to be the effect of magic. By the labour, industry, and spirit of the inhabitants, a forlorn waste has been converted into pleasant and fertile pastures, and they themselves have been rewarded by bounteous harvests. Drainage, embankments, engines, and inclosures, have given stability to the soil (which in its nature is as rich as the Delta of Egypt) as well as salubrity to the air.—*Chadwick, 2d Report*, Fol.

state of large towns,[1] says of the General Lying-in Hospital, in York Road, Lambeth: "The hospital was seldom free for any length of time from puerperal fever, occasionally producing frightful ravages, and requiring the building every now and then to be closed. After the greatest attention had been paid to cleanliness in every respect, the wards left open night and day for weeks, fumigated, the walls limed and painted, the beds thoroughly cleaned, fumigated, repaired, and frequently renewed, and the most scrupulous attention paid to cleanliness, the fever reappeared, on some occasions *immediately*, on the hospital being reopened. This circumstance made us look to external causes, when we ascertained that, in the immediate vicinity of the hospital, there were upwards of 1,500 feet of open ditches, receiving the drainage of the poor and dense population of the neighbourhood, one of the ditches being not more than thirty feet from the wall of the building. It was black and stagnant, and in constant ebullition from the disengagement of gas. After great difficulty and trouble, the hospital having to bear a large proportion of the expense, the Commissioners of Sewers were induced, although with much reluctance (in Oct. 1838), to have a portion of these ditches cleaned and properly arched over; an immense quantity of black pestilential mud was excavated, but, instead of being removed, it was spread over the adjoining ground. The effect was, that "at that time the hospital was freed from disease, but ten cases of puerperal fever occurred within twenty-four hours after this unjustifiable act." The drainage of the hospital was then carefully attended to, and ventilation also (the main drain had been entirely blocked up by two logs of wood), and puerperal fever ceased.

It appears, from the statistical accounts of Scotland, that the effects of drainage in various counties have been very beneficial to the health of the population.[2] There is not much intermittent fever in Edinburgh, but there was formerly a lock, called North Lock, which was famous for it. Ague, remittents, and intermittents prevailed in that district till the spot was drained.[3] They now no longer appear. The following facts are recorded in the Parliamentary Reports. In Fordown County: "So much draining that now no swamps; formerly, agues common; now quite unknown." Kin-

[1] First Report, 412.
[2] Second Rept. of the Lond. Commis. for 1848, p. 43.
[3] Elliotson's Lectures, Lond. Med. Gaz. ix. 890.

ross-Kinross: "Agues prevalent sixty years ago, in consequence of marshes; now never met with." Oswell: "Ague prevalent formerly, but not since the land was drained." Redgorton; "Healthy; no prevailing disease; ague was frequent formerly, but not since the land has been drained and planted." Albernyte: "Since the land was drained, scrofula rare, and ague unknown." Muckhart: "Great improvement in agriculture; ague formerly prevalent; not so now."[1]

Mr. Smith states, in answer to questions put to him, at the request of the Metropolitan Sanitary Commissioners, that in the alluvial clay district of Stirlingshire, and west of Perthshire, where the drainage was formerly effected by large open ditches, in the Dutch fashion, ague was periodically prevalent, and rheumatism, fevers, and scrofulous affections were much promoted, until the introduction of thorough drainage, forty years ago; after which period those diseases began to disappear, or to be greatly mitigated in severity. "Few cases of ague now appear. Fevers are seldom known, except in the usual course of fevers which prevail epidemically over the whole country. In the undrained condition of those districts, they were subject to dense fogs, especially in the autumnal months, when much rain had fallen, communicating a chilly feeling

[1] Dr. Southward Smith, before a committee of the House of Commons, declared: "That in every district in which fever returns frequently, and prevails extensively, there is, *uniformly*, bad sewerage; a bad supply of water; a bad supply of scavengers, and a consequent accumulation of filth. If you trace down the fever districts on a map of the Commissioners of Sewers, you will find that, wherever they have not been, the fever is prevalent; and, on the contrary, where they have been, fever is comparatively absent."

In the Holywell and Long Alley district, with five hundred recorded cases of diarrhœa and typhus, within the last six months, in one particular place, namely, Long Alley—which is a long, narrow, and close passage, crowded with inhabitants—the drainage, contrary to the general rule, has been put in good order, and this place has remained exempt from disease. Two courts in Whitechapel, that were in an exceedingly damp and filthy condition, and in which fever had prevailed for some time in every house, ceased to be affected with disease on being drained. In Dulwich, fever was arrested by clearing out drains. In Hammersmith, diarrhœa disappeared in one street after the emptying of a cesspool that had overflowed, a new drain having been built at the same time. In the Acton district, no fever is reported, and only ten cases of diarrhœa, the external and internal cleansing having been well attended to.—*Metropolitan Sanitary Commission, 2d Report* (1848), 22.

"It is reported to us that agues and remittent fevers, in Gravesend and the vicinity, have been greatly reduced after the drainage, although very imperfect, which has been accomplished in the town and neighbourhood.—*2d Report of London Commissioners* (1848), 42.

to the inhabitants; but since the general introduction of thorough draining, those fogs seldom prevail, unless in a general foggy tendency of the atmosphere of the country."

On the farm of Deanston, in the west of Perthshire, consisting of about 200 acres, and which was the first farm on which the entire system of thorough drainage and subsoil ploughing was introduced, there was a marked effect produced. The farm, after periods of rain, used to be covered with chilly fogs, which entirely disappeared after the thorough draining was effected.

Mr. Parker reports: "In respect of increased salubrity, induced in towns and rural districts by drainage, I may instance the acknowledged disappearance of ague and other periodical maladies consequent on the great drainage effected in Cambridgeshire, as in the Isle of Ely, &c., and in the Lincolnshire, and other great marshes. As an example of the good effects arising from the drainage of swamps, I may state that the Commissioners of Her Majesty's Woods and Forests, of which your lordship is the chief, have recently caused me to drain an extensive tract of country in the new forest, called the Wear's Lawn and Bog, adjoining which is a small hamlet, where inhabitants previously suffered much from intermittent fevers. The hamlet is now healthy; the offensive, gaseous emanations from the soil have ceased; and the inhabitants are supplied with an abundance of the purest spring water, discovered during the operations of drainage, and appropriated to their use."

A Mr. Neilson remarked: "I have also had several opportunities of witnessing similar effects in the West Indies, and particularly in British Guiana, where I resided several years. The surface is almost a dead flat, lower than the sea at high water, and drained only at considerable expense by large sluicegates for each estate, which are opened each period of low water. When an estate is abandoned, this is neglected, and its neighbourhood is invariably the first to suffer on the approach of an epidemic; and I have known instances of the course of a fever thus produced being checked, and materially altered, by the neighbouring lands being drained, an alteration considerably accelerated by a small quantity of lime, in a finely powdered state, being distributed on the lands during a windy day."[1]

[1] Drainage of the Land Forming the Sites of Towns, 69–72. London, 1852.

According to Dr. Graves, whose premature loss to the profession we must all greatly deplore, there can be no doubt that in Ireland, as in other countries, the effects of cultivation and drainage on the health of the inhabitants are very remarkable. He himself witnessed several exemplifications of the improvements thus effected in respect to the public health. Formerly, ague was of rather common occurrence in some marshy districts in the immediate vicinity of Dublin, and consequently, when he was a pupil, cases of intermittent fever were constantly to be met with in the hospitals; now, the low grounds have been drained, and thus the production of ague has been entirely arrested.[1]

The city of Cork, Ireland, lies in an irregular valley which is open to the east and west, but is confined by hills on the north and south sides. On the west it is connected with the fertile valley of the River Lee, which, rising from the mountain lake of Gongane Barra, flows in a devious course until it reaches the city of Cork, where it receives a large tidal accession. When the Lee is about a mile west of the city, it divides into two channels, which gradually diverge, but reunite again, thereby insulating a considerable tract, upon which the main part of the city is built. This tract is low, and being permeated by some small streams from the main branches, inundations occur when the river is swollen by heavy rains and high tides. From this cause the city anciently derived its not very flattering name, Corkagh, signifying a morass. This unpromising locality, from its marshy nature, was, as might be expected, very subject to intermittent fever, and "the fever and ague" were as familiar words in Cork as they are in the swamps of the Mississippi. Within the last century it has been drained, and in part built upon. The smaller inosculating channels have been arched over; the river has been confined by handsome quays to its two divisions, and the main branches have been deepened. The effect has been, the almost disappearance of the fever, which now shows itself but seldom, and for a number of years did not reign in an epidemic form.[2]

In fact, so far as Great Britain is concerned, the immense benefit of drainage has been proved by all the researches of Mr. Chadwick, who says: "In considering the circumstances external to the resi-

[1] Clinical Lectures, 51, Am. ed.

[2] Popham, Notes on the Climate and Diseases of the City of Cork, Dublin Journal of Med. Sc. xv. 290, May, 1853.

dence, which affect the sanitary condition of the population, the importance of a general land drainage is developed, by the inquiries as to the causes of the prevalent diseases, to be of a magnitude of which no conception had been formed at the commencement of the investigation; its importance is manifested by the severe consequences of its neglect in every part of the country, as well as by its advantages in the increasing salubrity and productiveness, wherever the drainage has been skilful and effectual.[1]

Lancisi gives a memorable instance of this in the town of Gondolpho, already referred to, which, notwithstanding its elevation, was rendered sickly by the exhalations from Lake Turnus, lying below it to the south-west. The lake was dried up, by order of Pope Paul V., and salubrity was the consequence.[2] The same writer relates an interesting case of the beneficial effect of drainage, which was communicated to him by Lentilius, physician to the Duke of Wurtemberg, and which occurred at Stutgard. "What you are preparing, on the noxious exhalations of marshes," says Lentilius, "cannot but be entertaining and useful. No one can doubt of the great importance of the subject. This I can illustrate by a single statement: Stutgard, the residence of my most serene Prince, is surrounded by mountains on every side except the east, where there is a sort of plain, stretching about an hour's walk towards the River Necker. Near the wall of the town, on the eastern side, and near that plain, was formerly a fish-pond or pool of several acres in extent. For many years, our metropolis has been notorious for its obstinate quotidian intermittents, which, being scarcely epidemic, but only endemic, have been commonly known by the name of the Stutgard fevers. It happened, about fifteen years ago, that this pool was drained, and converted into a most beautiful meadow. From that time, intermittent fevers have become so rare that they never once have deserved the name of epidemic, but only appear in sporadic cases, here and there, and are, moreover, much easier to cure. Hence, it appears that the fevers arose from the filthy effluvia of the pool, wafted by the eastern wind over the town, and confined there by the inability of the wind to sweep them quite away, on account of the mountains adjacent."[3]

[1] Second Rept. Fol.

[2] De Noxiis Palludum Effluviis, 20. [3] *Ibid.* 10.

M. Huguenin, in a Prize Essay on Ponds, which was presented to the Academy of Lyons in 1778, and is quoted in the Memoirs of the Royal Medical Society of Paris (vol. viii. p. 289), after dwelling on the injurious influences of marshes, says: "Lorraine long experienced these baneful effects, before the cause of the periodic return of the fatal febrile diseases, by which it was visited during several successive centuries, could be ascertained. While its humane and benevolent princes founded thousands of charitable institutions to insure relief against these ever-renewed evils; while natural philosophers and physicians laboured to discover the germ of those disastrous epidemic diseases, agriculture came to the aid of this desolated province, and, without suspecting the miracle it was about performing, drained, in a short time, two hundred ponds, in order to fecundate the precious soil which had so long been absorbed by the water. The air being thereby relieved of the humidity and putrid vapours by which it had been heretofore saturated, a stop, or at least an interruption, was put to this fatal circle of contagions, which, while endemic in localities surrounded by ponds, appeared often epidemically in the rest of the province."

In Paris, malarial fevers, in the time of Ballonius, were of daily occurrence, and prevailed on a large scale. They have now become so rare that when Alibert wrote his once popular treatise, it was with difficulty he could find more than a few cases for examination. Strasburg, already mentioned, has, in like manner, been *cured* of such diseases, except in some parts of the suburbs; but here, marshes have been allowed to remain unreclaimed. Like results, though not invariably so entirely satisfactory, have been obtained in other parts of France; as, for example, at Rochefort, Marenne,[1] Bresse,[2] Lorne,[3] Bourg,[4] Monbrison,[5] Feurs,[6] Lyons,[7] Bourgoin,[8] &c. If the first of those cities—which, in the days of Chirac, was proverbial for its unhealthiness, and was visited, in 1694, by a widespreading and malignant epidemic, of which that most distinguished physician has left us a vivid description—still suffers from fevers in autumn, the effect is due, not to the persistence of palludal influences within its walls, or in the immediate vicinity, but to other circumstances already adverted to.

[1] Melier, Mém. de l'Acad. de Médecine xiii. 673.
[2] Statistique du Depart. de l'Ain, 184; Fodéré, Méd. Légale, v. 251.
[3] Gaultier de Claubry, Mém. de l'Acad. de Méd. xiv. 129.
[4] Monfalcon, 32. [5] *Ib.* 42. [6] *Ib.* 184. [7] *Ib.* 182. [8] *Ib.* 182.

The present beautiful city of Bordeaux was so sickly formerly—so frequently and fearfully visited by periodic fevers, arising from the exhalation of extensive marshes, situate in the vicinity—that the Parliament of the Province, which held its meeting there, was often compelled to seek shelter in other and more favoured localities. Such was the case in 1473, 1495, 1501, 1515, 1525, 1546, 1653, and 1654; on which occasions, the disease assumed a more malignant form than common, and was, according to the custom of the times, held in the light of a plague. Similar events would, probably, have continued to occur, had not a venerable prelate, the Cardinal de Sourdis, then bishop of the Province, formed, in 1604, the project—which he carried into effect at his own expense—of draining and reclaiming the larger portion of the marshes situate near the city. This gigantic work was effected; and fevers, since then, have seldom if ever visited that city—never, in that part of it which had been more particularly under the influence of the culprit marshes. Other similar works, in other parts of the city or the vicinity, have been effected, and always with the same happy results.[1]

[1] Betbeder, Topogr. Méd. de la ville de Bordeaux, Mém. de la Soc. Roy. de Méd. i. 187; see also same work, viii. 279; Villermé, An. d'Hyg. xi. 349. The ponds of the Dombes, as already mentioned, have long been noted for the insalubrity they occasion. In 1839, the sickness in the vicinity was so great that a commission was appointed to suggest measures for remedying the evil. In his report on the subject, M. Purvis says: "Almost every individual examined admitted the baneful effects of the ponds; a large number viewed them as the principal cause; a small number referred the effect to the nature of the soil; others, to the marshy condition of the fields, &c." At the same time, a large amount of facts were adduced in the course of the inquiry, tending to show that the salubrity of the place was restored whenever the ponds were dried up. The commission, in view of the numerous facts obtained during the inquiry, —influenced by the advice of all the physicians who have turned their attention to questions of public health, and of all the agriculturists and economists who have written on the subject—taking into consideration that the Dombes, before the increase in the number of ponds, was much more extensively cultivated and more populous than since they have been made to occupy a large portion of the soil; that since then more than one-half of the population seem to have disappeared; that while the Bresse, the formation of which is similar to that of the Dombes, with an inferior and unhealthier soil, has, by drying up its ponds, enhanced its prosperity, and possesses now a population of 1,600 inhabitants to every square league; the Dombes, on the contrary, by multiplying hers, has lessened greatly the net value of her agricultural products, and has now a population of less than 400 to the square league;—considering, finally, that insalubrity and fevers reappear everywhere with the increase of ponds, and that commonly salubrity is everywhere restored when these are dried up, the commissioners are unanimous in the opinion that the ponds are, without the least doubt, the principal cause of the insalubrity of the Dombes." They admit that marshy fields, bad

Surrounded as Copenhagen and all Denmark are by water, salt and fresh, and situate, as the former is, in a low and flat country, containing much stagnant water and many ditches, it is no wonder that ague is of very common occurrence there. Yet the disease, as we learn from Professor Otto, "is much less frequent now than formerly, which must be attributed to the draining of several pools and stagnant waters. The ague was, at one time, from the want of proper drainage, extremely frequent, and in many cases fatal, on the island of Langland."[1]

Around Scanderoon, the seaport of Aleppo, are extensive marshes and swamps, which produce their usual effects. "During the sway of Ibrahim Pasha in the country, it was brought before his notice. Attracting as it did so much attention, the whole was drained, and canals formed to carry off the waste water continually from the hills, &c. For two years subsequent to the completion of these salutary exertions, an almost perfect immunity was enjoyed; but the soldiers, at the commencement of the operations in 1840, destroyed the embankments, &c., and soon reconverted what had become cultivated land into their former state, and the disease, which had almost been banished from the neighbourhood, again resumed as severe a sway as formerly."[2]

As cities enlarge and improve, malarial fevers decrease in them.—In our own cities—the larger ones particularly—remittent and inter-

regimen, and, perhaps, the nature of the soil, have some agency in producing that effect; but they believe that all those causes combined are far from being capable of occasioning a degree of evil equal to that occasioned by the ponds. (Statistique du Department de l'Ain, quoted from Becquerel on Climate, 278.) The commune of Varengeville, in the arrondissement of Dieppe (Seine Inférieure), is situate on a surface which formerly constituted part of a large marshy district noted for its unhealthiness, and was the frequent seat, during the autumn, of epidemics of intermittent fever. On one occasion, the almost entire population—177 out of 182—suffered simultaneously or in succession. Their general health was bad; they presented signs of a cachectic state, and all the morbid conditions resulting from frequent attacks of intermittent fever. The duration of life among them was short. Considerable ameliorations have been effected in the condition of this locality during the last sixty years. A great part of the marshy surface has been drained and converted into cultivated fields, and periodic fevers have diminished or disappeared. In some parts, however, and especially in the small commune of Varengeville, little or nothing has been done to reclaim the land and marshy surfaces, and stagnant pools continue to exist; and wherever such is the case, fevers continue to prevail. (Mém. de l'Ac. de Méd. xiv. 118.)

[1] Topogr. of Copenhagen. Trans. of the Provincial Med. and Surg. Assoc. vii. 205.

[2] Robertson, Med. Notes on Syria, Ed. J. lx. 38.

mittent fevers are limited to the suburbs. As the buildings extend out, and the closely inhabited portions expand, and, by so doing, lessen the area of humid and exposed soil, the disease recedes. Charleston, Savannah, New York, Buffalo, Auburn, Geneva, Syracuse, Salina, and Louisville, &c. may be appealed to in illustration of this. The statement of Professor Yandall, relatively to the latter city, are apposite. The rock, of which the subsoil is composed, "forms a surface remarkable for its evenness; and the soil which it produces, as it crumbles under the action of the air, frost, and water, is peculiarly retentive of moisture. Ponds and slushes are abundant, wherever the black slate constitutes the surface-rock. The first houses erected at the fall were built in the midst of ponds. Entire squares of the city are now pointed out, which occupy the ancient beds of ponds, large and deep enough to float a steamboat. These have all been drained, and such collections of water are nowhere to be seen within the city limits." Louisville, while it stood amid its ponds, Dr. Yandall remarks, "was regarded as one of the most sickly towns in the Valley of the Mississippi. It was commonly called '*The Graveyard of the West.*' It is now esteemed one of the most healthy. Intermittent fever was a regular annual visitant, and occasionally a form of bilious fever prevailed, rivalling yellow in malignity, and threatening to depopulate the town. The most fatal of these endemics broke out in 1822, after a hot, rainy season. The number of victims from it, out of a population of less than five thousand, was two hundred and thirty-two. In a family consisting of twenty persons, nineteen were sick at one time, and, in some families, every individual died. At this time, only one street in Louisville was paved, and within its limits were at least eight ponds of greater or less dimensions, most of which, in the course of the autumn, were dried up, exposing foul bottoms to the sun. "The ponds have all disappeared. The streets have generally been paved, and, though the grading is defective, and can never be as effectual for drainage as it might be rendered on a less even surface, still, it is such as to carry off the rains into the river, and the ditches south of the city. The only parts of Louisville obnoxious to the charge of unhealthfulness, are its suburbs. Beargrass, a small sluggish stream, with alluvial banks, which empties into the river at the foot of Third Street, taints the air in its neighbourhood. The ponds send up their effluvia from the south; and the extended rocks of the falls, laid bare by the retreating river in dry seasons,

exhales the poison of intermittent fever. The inhabitants along the Beargrass, and of Water Street, and of the scattered dwellings in the outskirts of the city, are sorely afflicted with the fever. The infected circle is receding. In 1837, it was not deemed safe to reside nearer the limits of the city than Walnut Street. Now, Chestnut Street is considered healthy. As we go from the suburbs towards the crowded parts, the chances for health increase, and, as the new streets are built up, a barrier to the fever poison is thrown around the older neighbourhoods."[1]

If the reader desires to have another convincing proof of the cessation of the production of autumnal fever from changes of the kind mentioned, let him come and see what has been the result in Philadelphia. In former days, when the city was of limited extent—with few improvements—with buildings scattered about, and leaving open and unimproved spaces between them—with a marshy stream running through the greater part of it—with ponds, natural and artificial, spotted over the plot in various directions, and with unpaved streets—fever was of common occurrence, and epidemics were not unfrequent. At present, malarial fevers are unknown in the city proper, as well as in the compactly built and well-drained portions of the suburban districts. If we wish to meet with them, we must go to the outskirts of these districts, or to some distance from them, to the open meadow ground of the neck, or to other unimproved surfaces of the vicinity; but, more particularly, to the marshes which still exist along the river banks.

Our townsman, Dr. Emerson, who has devoted considerable attention to the subject of public hygiene, and furnished several excellent essays on the vital statistics of Philadelphia, has conclusively shown that the influence of the sickly air is expended upon the comparatively limited portion of the population living in the environs and outskirts of the city. During the periods embraced in his calculations—and the same holds good in all other times and places—the fever, in some of its forms, was almost universal; whilst, in the more dense and well-paved parts, the air seemed unusually healthy; and where remittents and intermittents were met with, they could almost invariably be traced to exposure to night air in the country or suburbs. Never, he remarks, was a stronger demonstration

[1] Sanitary Condition of Louisville; Trans. of Med. Assoc. ii. 611, 612; see also Drake, 249.

afforded of the resistance made by cities to the influence of country malaria, than our late experience has furnished. Great as was the amount of sickness during the epidemic of 1822 and 3, it was confined almost entirely to the comparatively small proportion of population inhabiting the unpaved or ill-paved environs. "Our observation," Dr. E. adds, "on this and other occasions, has led us to ascribe this exemption, for the most part, to the pavements, which, by effecting a perfect drainage, prevents exhalation, at the same time that it admits of the total removal of vegetable and animal matters, the sources of foul and unhealthy emanations. The chief motive for paving the streets and sidewalks is usually convenience; but it has always appeared to us that by far the most important object achieved by it was the preservation of health."[1] Since the time to which this has reference, thirty years have elapsed. In the interval, the compact, dense, and well-paved parts have extended far beyond where they were then, and with their expansion the disease has receded.

Hear what a physician of the middle of the last century says: "When I first came into this city, the dock was the common-sewer of filth, and was such a nuisance to the inhabitants about it, that they were obliged to use more pounds of bark, than they have ounces, since it has been raised and levelled."[2]

Hear also what Dr. Bond's distinguished contemporary, Cadwallader Colden, states: "I remember that several years since, when I was at Bristol, Pa., opposite to Burlington, which is situated to the northward of a large space of swamp ground, they told me that they had been, from the first settling of Bristol, subject to intermittent fevers of a malignant kind; and, indeed, the aspect of the inhabitants showed the ill effects of the air which they breathed. While I saw them, they assured me, at the same time, that not

[1] Amer. J. ix. 27.

[2] Bond's Introductory Lecture, delivered at the Pennsylv. Hospital, Dec. 17, 1776; N. A. Med. and Surg. J. iv. 270.

During the winter of 1849, 1850, the New Orleans city authorities ordered all the timber standing between the city and the Métairie ridge—an elevated space between the city and lake, in the midst of a cypress swamp—to be cut down. The greater part of the ground was also well drained. By this means, an extensive swamp was dried up by the rays of the sun, and the obstruction to free ventilation, by the breezes of the lake, removed. Writing a year after, Dr. Fenner says: "I think it not at all improbable, that this work has already exerted a beneficial influence upon the health of the city."—*Southern Medical Reports*, ii. 83.

above two or three children, born in that village since its first settling, had attained to the age of maturity; but since that time, these swamps having been drained and converted into profitable meadow grounds, I am informed that Bristol is, in a great measure, freed from these annual epidemical fevers."[1] Speaking of Cahawba, Ala., Dr. English states that the land in the immediate vicinity of the town is low and flat, and was, until 1840, interspersed with numerous small shallow ponds. These have been thoroughly drained, and now hold water but a few hours after the heaviest rains. It has become a subject of remark by the citizens of the place, that since that time the town has become much more healthy, particularly as regards endemic fevers. In truth, in the last four or five years, the former dreaded scourge, bilious remittent fever, has almost disappeared, and the few cases that do occur are of a mild type.[2]

Beneficial effects obtained from the draining of marshes prove the existence and morbific agency of malaria.—Large portions of the Tuscan and Luccan Marshes, the maremmes of Albagna, of Ombrone, Grossetto, Orbatello (between Sienna and the sea), Campilla, Piombino, Castiglione, the val di Chiana,[3] have been reclaimed; the soil has been improved, and applied to useful purposes, and there sickness has disappeared. The reader will find, in Tozetti's travels in Tuscany,[4] an interesting account of the important changes effected by the Grand Dukes Cosmo I. II. and III., and Ferdinand I. and II., with a view to reclaim the marshes, and destroy the insalubrity of the country around Pisa, and of the satisfactory results thereby obtained. At a later period, under the direction of Gaetano Georgini, and by order of Dukes Leopold I. and II., the principal basins of the Tuscan Marcmmes were drained, and otherwise improved, and there, as was expected, fevers have disappeared, and the population has sensibly increased. The town of Massa, which, for years, was noted for its insalubrity, was completely restored to a healthful condition by the draining of a large and deep marsh, situate on the north-east of it. By means of a canal, constructed through one

[1] Med. Regist. i. 323. [2] N. O. J. vi. 168.

[3] Fossombroni, Mémoires historiques et hydrauliques sur le val de Chiana, 1789; Tartini memorie sul beneficiamento delle Maremme Toscana, 213; Carrière, 325.

[4] Voyage Minéralogique, Philosophique, et Historique en Toscane, par le Dr. J. T. Tozetti, i. 392.

side of the hollow, the water escaped, the marsh was drained, and the morbid effect was arrested. A few years after, the canal, which was too small, being choked up, the surface once more became marshy, and fevers reappeared; but, in 1829, the same canal was cleansed and widened; the soil was again dried; fevers once more ceased to prevail, and the city has, since then, remained healthy.[1]

As regards the Luccan Marshes improvements had been effected in them as early as 1741, when one of the three basins, into which they are divided—the Massaciuccoli—the most unwholesome of all, was drained. The success was so complete that fever entirely ceased. In 1768, 9, the sea-water, owing to a derangement in the locks, once more covered the reclaimed land. As a natural consequence, marshes were reformed, and, with these, the disease reappeared. The mortality at Viareggio, and the adjacent parishes, which the year before had not exceeded one in forty of the population, amounted now to one in fifteen (170 in 1330). The locks were repaired, and health was re-established. In 1784, 5, a like accident in the locks occurred, and the same results followed—fever returned. In the former of these years, out of a population of 1,898, 1,200 were attacked, while the number of deaths was 92, or 1 in 20 of the whole amount of the inhabitants. In 1785, 103 died, out of a population of 1,834, or 1 in about 18. Again, the locks were repaired, the sea was excluded, the marshes were dried up, and health was once more restored. Other portions of these pestiferous localities were reclaimed in 1812, 1819, and 1821, and everywhere the success has been equally satisfactory.[2] Need I add that, with the diminution or cessation of fever, the population has increased? At Grosseto alone, from 1814 to 1843, the augmentation has been from 53,175 to 76,179.[3] In Viareggio, the number of inhabitants in 1733 was 1,509; in 1823 (90 years), it had increased to 9,408.[4]

On the opposite effect, the following remarks of Lancisi deserve attention:—

"And we, taught by the calamities of the noble and crowded cities of Italy, which have been desolated by the marshes alone, behold with our own eyes the mischiefs occasioned by stagnating waters, without looking abroad for the reasons or inquiring for the

[1] Carrière, Le Climat de l'Italie, 324, 5; Salvagnoli, Saggio delle Statistica medica, delle Maremme Toscane.

[2] Giordini, An. de Chemie, &c. xxix. 225. 234; Mélier, *op. cit.* 686.

[3] See Carrière, 333.

[4] Giordini, *op. cit.* 237.

evidence of other persons. Aquileia, for example, formerly in a most flourishing condition from the number of its inhabitants, even so much as to merit its advancement, first to a metropolitan and then to a patriarchal dignity, scarcely retains at this day the remains of houses, or any traces of its former splendour; and the sole cause of its overthrow has been the contamination of the air from undrained waters. The city of Brundusium, formerly so famous, is lamentably mentioned by Antoninus Galatheus, in words which, by reason of their aptness, we quote: 'Moreover, cities situated in a healthy climate have been destroyed. Cities, indeed, like men, have their vicissitudes. But the neglect of its inhabitants has been the ruin of Brundusium; for had outlets been made for its waters, it would never have acquired such an unhappy distinction.'"[1]

The effects of covering sickly places with water lead to the same belief.—But draining, improving, and cultivating the surface of infected localities are not the only modes of putting a stop to, or greatly diminishing, the prevalence of fever, and thereby proving the fact that these must arise from a cause exhaled from the soil. We have seen, on the authority of Pringle, that, during the campaign of 1748, in Brabant, the country bordering on the lower part of the Maas, was rendered more unhealthy, upon letting off part of the water by which the country around the fortified towns had been submerged. It may now be proper to add, from the same author, that the "States of Holland, being made sensible of the sickness which raged at Breda, and in the neighbouring villages, gave orders to let in the water again, and to keep it up till winter."[2] Indeed, experience taught Pringle that, "as to cantonments in marshy grounds, if the troops must remain there in the dangerous season, it will be better to float the fields entirely, than to leave them half dry."[3] The same process was resorted to some years ago, with complete success, at Paris, Bordeaux, and other parts of France, and is never neglected there, when marshy surfaces cannot be otherwise reclaimed.[4] We have already seen the means adopted to correct the blunders of the revolutionary government, in 1793. Blane informs us that, while the British troops were suffering from

[1] Lancisi, *op. cit.* lib. i. cap. iii. 8; see also Dr. Mitchell's Tr., Med. Repos. xiii. 16.
[2] Pp. 61, 62. [3] *Ibid.* 98.
[4] Parent du Chatelet, An. d'Hyg. xi. 310; Fleuriau de Bellevue, Comptes Rendus de l'Acad. des Sc. xxv. 338, 9.

the Walcheren fever, "the native inhabitants affirmed they were less sickly than usual at the same time, owing, as they said, to the unusual quantity of rain that had fallen during the two or three preceding months."[1]

Indeed, the beneficial effects obtained from the flooding of marshes or insalubrious surfaces, by artificial means, by freshets, or otherwise, have been noticed from time immemorial, as well in foreign countries, as in our own.[2] The practice is certainly not new, and may be traced to an early period in the annals of hygiene, as the reader will find by reverting to the history of Empedocles, who delivered the inhabitants of Salimonte of the effluvia exhaled from the marshy banks of the neighbouring rivers, by causing these to be flooded by means of the pure water of other streams. In many of the islands of South America, on the coast of Mexico, at Batavia, in Africa, in the East and West Indies, in Europe, and elsewhere, sickness, as we have seen, prevails at the commencement and close of the rainy season, and ceases completely, or in great measure, when the rivers are high, and the marshes, swamps, as well as the country generally, are covered over with water.

More than a century ago, Desportes expressed himself in favour of the opinion that yellow fever arises often from the foul condition of ships, and attributed the escape of a vessel, the Jason, of seventy-four guns, in 1746, at St. Domingo, to its extremely leaky condition, by which the sources of exhalations in the hold were submerged.[3]

The common saying of the sailors, that a leaky ship is ever a healthy ship, is well known; and, in conformity with the result of experience on that point, the submerging or sinking of infected vessels has not unfrequently been resorted to as a means of purification.

The practice is noticed, as extremely useful under peculiar circumstances, by Lancisi, who was too clear an observer not to have discovered that marshes are "harmless, when plentifully diluted and cleansed throughout by pure and fresh supplies of water." Lancisi reminds us, too, of a fact mentioned centuries ago, by Strabo,[4] and which has been already briefly alluded to: "Alexandria, in Egypt,

[1] Dissertations, i. 219.

[2] Carrière, *loc. cit.* 526; Ludlow, N. Y. J. ii. 84; Caldwell, Essay on Malaria, 75; Villermé, An. d'Hyg. ii. 349.

[3] Histoire des Maladies de St. Domingue, i. 162.

[4] Geographia, lib. xvii. 278.

stands near the marshes; and although it ought to experience, during the scorching heat of summer, a close and suffocating air, yet, by the seasonable rise of the Nile, nothing filthy exists whence vitiated exhalations can arise, especially when the Etesian winds begin to blow from the north, and arrive after sweeping a wide tract of sea."

The surface now covered by the city of Rome, as well as that over which was spread the ancient mistress of the world, and the adjacent campagna, could scarcely fail to be, to a certain extent, insalubrious in olden times; but, from all we can collect on the subject, it would appear to have been less so formerly than it is at the present day. When limited to a few hills, the Quirinal, the Palatine, and the Capitoline, marshes of large size—the great and little Velabrum—existed in the close vicinity. At no great distance, were other marshes, the Capuan, and those of Tarentum. Then there were the Lakes of Castiglione and Giuturna, besides smaller lakes or pools situate all about the Roman territory. Most of those localities, though now uninhabitable, were then populous, and, in all probability, owed their greater salubrity to the fact that the larger number of those receptacles of water, which were then completely filled, have been partially drained by artificial means, or filled up by the gradual increase of their subaqueous soil, and retains beneath the surface the water by which it was once covered.

The following fact, already adverted to, illustrates more than one of the results here mentioned: "It has been remarked by persons who live in the vicinity of Morne Fortune (St. Lucia), that when the military, who inhabit the Morne, suffer severely from sickness, the inhabitants of the town of Castrie (in the close vicinity), are generally in good health; and, on the contrary, that when the inhabitants of the town are sickly, the garrison on the height is comparatively healthy. The Morne is a bog in wet weather; Castrie is then an inundated swamp; in continued dry weather, the Morne has a hard and firm surface; Castrie is then a swamp advancing to exsiccation. The fact is obvious, and presents itself as a cause of what takes place."[1] All these facts, as also the stoppage of febrile epidemics by a profuse fall of rain, are, besides many referred to under another head, attested by the most reliable authorities.[2]

[1] R. Jackson, Sketch, ii. 358, 9.

[2] Bally, 309; James Johnson, 43, 320, 330; McWilliams, 184; Pritchett, 108; Boyle, 3, 123; Dazille, 10; Desportes, i. 52, 80, 87; Firmin, 3, 17; Gillespie, 137;

To the same effect may be cited the beneficial results obtained in France and elsewhere, by the filling up of ditches and other excavations—remarkable examples of which are on record.[1] I might dwell on the effects produced by the covering of the marshy margins of river shores by *sand* inundations, as observed on the borders of the Baltic, in Holland, Italy, France, Africa,[2] &c., and particularly on the well-known case of the Goodwin Sands, in which, while, from a similar cause, the usefulness of the land was destroyed, the salubrity of the vicinity was firmly established. I might also point out those instances, in which the infection of a locality has been remedied, by covering the focus of exhalation with earth, as was done to Galiopolis, in 1796.[3]

H. McLean, 25; Rush, iv. 154; Ferguson, Recol. 199; Chisholm, i. 294; Ferguson, Med.-Ch. Tr. viii. 180, 1; Lempriere, i. 26; Rochoux, 11; Caillot, 121; Valentin, 89; Gouraud, 65; Arnold, 31; Furlong, Med.-Ch. Rev. xxv. 289; Dickson, Edinb. J. xiii. 47; Bancroft, 200; Berthe, 156; Macculloch, 204–6; Brown, in Cyc. of Pract. Med. iii. 61; Cooke, Med. Rec. vii. 457; Pinckard, ii. 485, 6; U. Parsons, 204; Irvine, Dis. of Sicily, 6; R. Jackson, Sketch, ii. 259.

[1] Monfalcon, 43; Macculloch, 126, 7; Cycl. of Pract. Med. iii. 61.

[2] Macculloch, 207.

[3] Potter on Contagion, 16.

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CHAPTER IV.

EXISTENCE AND MORBID AGENCY OF MALARIA, CONTINUED.

The effect of the "washing" of sickly places lead to the same belief.—In the preceding chapter, attention was called to the salutary effects of perfect drainage, as illustrative of the existence and morbid agency of a malarial poison. With the same view I may, besides, appeal to those instances in which places, heretofore insalubrious, have been rendered otherwise, by being thoroughly *washed*, through the agency of a freshet, or an inundation, which carried off all substances susceptible of decomposition, and left in their stead a deposit of innocuous materials—as occurred in New Orleans, after the crevasses of May, 1816, and 1849,[1] and more recently in some parts of the State of Pennsylvania. In the latter instance, prior to September, 1850, intermittent fever prevailed to a great extent along the course of the Schuylkill, and was found, in many instances, to be unmanageable, showing a tendency to a frequent recurrence. But, since the flood which took place at the time mentioned, the same localities have been remarkably free from it.[2] A flood, the highest that ever occurred in the same river, took place on the night of the 2d and 3d of September, 1850. "It completely inundated a small village, of about thirty houses, in our neighbourhood, besides some farm-houses. It filled, of course, all the cellars and wells, and it left besides a great deal of filthy rubbish, forming a deposit of mud, of several inches in thickness, of a most offensive character. The greater part of two weeks must have elapsed before the cellars could be cleansed, and the wells be fit for use. Meanwhile, the people were living in filth, and without the usual supply of pure

[1] Transactions of the Pennsylvania State Medical Society, ii. 34; see also i. 24.

[2] Nicol, Obs. on the Nature of the Climate of Seringapatam, Edinb. J. xi. 290; J. Johnson on Trop. Cl. 101.

water, and the surface of the ground everywhere about them covered with the mud above alluded to. Yet, I am not aware that this state of things had the least influence in aggravating the fevers then prevailing. I asked the wife of a farmer, who had suffered as much from the flood as any other person, and in whose family intermittent fever prevailed, what effect the flood had had upon the disease. Her answer was: "I think we have had less of the ague since the flood than before it."[1] In this case, the materials of malaria were washed away in some parts, and new innocuous mud was left in its place.

We learn from Vitruvius, that, in the lagunes of Venice, the air was extremely pure, especially around Ravenna, Altino, and Aquileia, the three principal, and doubtless the most populous points. He himself affords us the explanation of this anomaly. At that remote period, the marshes and ponds were situate to the north and east of those districts, and their bottoms were higher than the level of the sea; owing to which the latter, at every ebbing of the tide, carried away all the putrescent materials and filth, and thereby cleansed the lagunes.

In Seringapatam, materials of putrefaction, for about eight months of the year, lie all over the banks of water streams and other repositories, "until the periodical rains of Malabar begin, which, falling in the Ghauts, run down, and fill the Cauvery River. The filling of this river is always very sudden, and it comes rushing along with great impetuosity; sweeps out all the filth from the ditches; clears away all the impurities, so long stagnant on the island; and leaves the place, for a while, tolerably healthy, and the air cool and refreshing."[2]

The city of Avignon was inundated on the 30th of October, and the 4th of November by a rise of the Rhone. Nine-tenths of the city were under water. No fever, however, followed, owing to the complete washing which the surface underwent, and the supervention of the cool north winds, which wafted the morbid exhalations along the great valley of the Rhone out to sea.[3]

With equal propriety, I may call attention to the results obtained in some parts of the Pontine Marshes, as well as in and about the

[1] Pennypacker, of Chester County, Tr. of Penn. St. Med. Soc. i. 69.

[2] J. Johnson on Trop. Cl. 101; Nicol, Obs. on the Climates, &c. of Seringapatam, Edinb. J. xi. 290.

[3] Gouraud, Fièvres Interm. Pernicieuses, 133, 4.

Eternal City.[1] In former days, that part of Rome on which the immense population was crowded, and which is now almost deserted, was healthy—comparatively so, at least—while the insalubrious sections were the Campus Martius, the Velabrum, and other parts bordering on the river;—the site of the modern city. The reverse is now the case; for, as we approach the inhabited parts of the present city, through the space separating St. John, of Lateran, from the Forum and the Velabrum, we pass over the principal focus of the pestiferous exhalations. On the other hand, the surface of the Campus Martius, or indeed the whole valley, is free from the tainted atmosphere. The very section appropriated to the Jews, the Guetto, where the principles of public hygiene are sadly neglected, is, to a very great degree, healthy. How has this happened? The Campus Martius was purified by Leo X., and the surface, after being divided into streets, was soon covered with houses, churches, and other buildings. The population, at the close of the reign of that pontiff, had already reached 60,000. The narrow valley, between the Tiber and the Pincian Hill, by which we now enter Rome, was transformed from a vast marsh into the beautiful Piazza del Popolo; and other portions were, by successive pontiffs, greatly ameliorated.[2] The site of the old city, which was not, originally, favourable to health—both on account of the peculiar condition of the soil, and its exposure to the influence of distant sources of miasmal infection, but which had been rendered much less hurtful by drains, the erection of numerous aqueducts, and other works of kindred character—has returned to its pristine state. It has gone to destruction, and is now deserted. The houses and monuments by which it was covered have disappeared; the greater number of the aqueducts have been destroyed, with the effect of allowing the free escape of the water, and the formation of marshes and pools; the drains have been choked up, and the whole surface presents a mass of ruins and rubbish.[3]

Much more might be said on this important subject; but the instances mentioned must suffice. They furnish us with both proof and counter-proof. With the existence of marshes, or marshy and partially drained land, we have fever; with the re-

[1] See Carrière, Climat de l'Italie; and Tournon, Etudes sur Rome.

[2] Carrière, *op. cit.* 161.

[3] See Review of Carrière, by the present writer, Am. Journ. of Med. Sci. July, 1851, p. 163.

claiming of that land, or its complete overflow, fever disappears. Whenever, as in some cases cited, the surface returns to its pristine marshy condition, the unhealthiness of the country returns also. Once more the marshes are drained, and, with the improvement of the land, we have a cessation of fever. In some instances, the disease is found to break out in impure city localities; the effect is remedied by a judicious employment of drains, sewers, and other kindred means. Nothing, it appears to me, can be more satisfactory; because no one who has looked at all into this matter, can have failed to perceive that, of all diseases which are susceptible of assuming an epidemic character, few or none exhibit so clearly the close relationship between the existence of certain local agents and the effects produced by them, as periodic fevers; none the productive causes of which are more strikingly and undeniably under the control of human agency. And when, bearing these circumstances in mind, we revert to the fact that the mixture of salt and fresh water has been found to increase[1] greatly the unhealthiness of marshy surfaces—a result foreseen by Vitruvius,[2] pointed out more satisfactorily by Lancisi,[3] dwelled upon by Gul. Piso,[4] Sir J. Pringle,[5] and Sylvius;[6] noticed more than a century ago in this country by Cadwallader Colden,[7] and subsequently by Ludlow,[8] and other American writers; and confirmed, beyond dispute, by the effects observed at Bender-Abassi, on the banks of the Persian Gulf;[9] at the Valdue;[10] at Martigues; in the Luccan Marshes, near Viareggio, Motrone, and Montignoso; at Sebgha (Algeria); in South America, &c.;[11] it will be difficult to resist the conclusion that some

[1] "There dreadful is the air in low places, near the sea-shore, into which the waves find entrance by a canal that has either been open during the memory of man, or made by human artifice, or produced by a storm; and into which receptacle also the rains wash down the filth from the adjoining knolls and hills." "There are some ponds of the kind in the territories of Ferrara and Ravenna as well as of Rome, especially near Ostea, where the salt-works have been erected."—*Lancisi, op cit.* 18.

[2] De Architectura, lib. i. cap. iv.

[3] De Nox. Palude Effluviis, 16, 18.

[4] Hist. Nat. and Med. lib. i. 9.

[5] Diseases of the Army, Appendix, 370.

[6] Pract. Med. Append. Tract. x. 191.

[7] Account of the Fever of New York in 1741, 42. Med. and Philos. Register, i. 323.

[8] New York Med. and Phys. Journ. ii. 85.

[9] Chardin, Voyage en Perse; Fodéré, Méd. Lég. v. 169.

[10] Fodéré, *ib.* 168.

[11] Giordini (Gaetano), Causes de l'Insalubrité de l'Air dans le voisinage des marais en communication avec la mer. Ann. de Chimie, xxix. 226; Carrière, Climat de l'Italie, 279; Boussingault, Ann. de Chimie, lvii. 150, 151; Salva (in Rochoux), 129;

poison exhales from surfaces where the mixture occurs, and that the disease which ensues or prevails more widely there is the effect of it, and not simply of heat, moisture, atmospheric vicissitudes, or other agencies of the kind.

The conclusion will appear the more natural when we learn that, while such is the effect of the mixture in question, it has been found that, in the salinas of France, fevers are limited to the edges and immediate vicinities of the rivulets, ditches, and canals surrounding or running through the salt basins, but containing fresh water; and do not affect the extensive surfaces on which unadulterated sea-water is introduced for the purpose of evaporation, and which, at a certain period of the process, assume the outward characters of ordinary marshes;[1] when, besides, we find that, so far from these pure salt marshes proving injurious, the physicians of Marseilles (Mercandier, Robert, Peyron, Girard, &c.), to whom the subject was referred for examination, have pointed out, and experience has demonstrated, the benefit resulting from the conversion of common paludal localities into regular salinas, as may be found by referring to the oft-quoted Report of Mélier, contained in the 13th vol. of the *Memoire of the Academy of Medicine* of Paris, p. 655; and that, conversely, the change of a salina into a common marsh, has been followed by the appearance and wide prevalence of fever, which heretofore had not existed—a change which has caused the almost total depopulation and destruction of the once flourishing cities of Marenne and Brouage, in France.[2] Dr. Ludlow, of New York, remarks that the results of his observations, made at the villages of Salina and Montezuma, New York, where this combination takes place is, that it depends entirely on the proportions in which the waters are mixed. If the waters of fresh marshes are largely combined with salt water, or *vice versâ*, the general healthiness of the situation will be improved. Dr. Hosack mentions, in his lectures, that the marshes of Hoboken, in New Jersey, while overflowed by the sea-water, were healthy, intermittent and remit-

Trail, Outlines of Medical Jurisprudence; McWilliams, 160; Daniel, Med. Gaz.; Mélier, Mém. de l'Acad. xiii. 684; Monfalcon, 69, 70; Thouvenelle, Climat de l'Italie, i. 180, 183; iv. 34, 83, 218; Sigaud, du Climat et des Maladies du Brésil, 172; Morichini, 32.

[1] Mélier, *op. cit.* 635, 636, 684; Jacquot, *loc. cit.* 21, 25, 47; Maillot; Bourdier, in same; Macculloch, 37, 8.

[2] Mélier, *op. cit.* 642, 645; *ib.* 651, 665.

tent fevers being unknown; but since they have been drained, and the ingress of the salt-water has been prevented, these diseases are endemic. This was farther proved by the breaking of the dykes during their prevalence, when they immediately ceased.[1] When, again, we bear in mind that the morbific effects produced in the localities mentioned are, sometimes at least, circumscribed, like those noticed on shipboard, within very narrow bounds; that while in vessels the area of the infection, which necessarily can never be large, is as already stated often limited to one side or one end of the under-decks, or to the vicinity of the pumps, where, indeed, it usually commences, the disease on land is frequently confined to the immediate neighbourhood of a pond, of a mill-dam, of a sluggish stream, of masses of decaying or green timber, or of other vegetable or animal substances, separate or combined, in a state of decomposition, or of a drain, or cesspool[2]—not seldom to one side only of these; to an overflowed field or bank; to a few streets of a city, to one single street, or even to one side of this;[3] to a few buildings; to one solitary house, or side of a house, or room, or corner of a room,[4] or the like; we cannot well see how all these facts—the reality of which will not be contested—can be explained on the principles set forth by the opponents of malaria. The degree of heat and humidity, the extent of the dew-point, the amount of atmospheric vicissitudes and other morbid influences to which they look for the causes of the disease, can scarcely be supposed to be so much greater on one side of a street, in a few buildings, in a solitary house, in one room, or corner of a room, than on the other

[1] *Op. cit.* ii. 85; Hosack's Practice, 175.

[2] Rush, iv. 96; Potter, 16; Caldwell, Dissert. 491, 2; *ib.* Essay, 69; *ib.* Fever of 1805, p. 60; Picornel, in Thomas, 8, 22; Usher Parsons, Essays, 215; *ib.* Hays's Journ. vii. 80; *ib.* Boston Med. Journ. iii. 674, 688; Rand, Med. Reposit. ii. 467; Valentin, Voy. Med. 55, 6; Vicq d'Azyr, on City Interments; see Review of this work in Chapman's Journ. vii. 266–8; Audouard, Archives, xii. 312; Tr. of Med. Soc. of State of Pennsyl. ii. 51; Harris, Charleston Med. Journ. ii. 615; Second Report on Quarantine (Lond. 1852), 67, 68; Gaultier de Claubry, Mém. de l'Acad. de Méd. xiv. 118.

[3] Lancisi, *op. cit.* 154; Bancroft, 165; Berthe, 74; Baglivi, 158; Rigault de l'Isle (in Johnson), 315; Amer. Rev. iv. 296; Faust, Amer. Journ. vi. 53; Trans. of Med. Soc. of State of Pennsyl. ii. 93; Macculloch; Lefoulon, p. 60, note; Pendleton, Charleston Journ. vii. 450; Becquerel, sur les Climats, 10.

[4] Lancisi, 154; Ferguson, Med.-Chirurg. Trans. viii. 143; Callow in Cycl. ii. 280; Wilson, 158, 9; Vatable, 344; Rufz, 28; Imray, Edinb. Journ. lxiv. 355; Davy, Topog. of Medit. ii. 248; Harris, Charleston Journ. ii. 615; Booth, Life of Armstrong, ii. 721; Second Report on Quarantine, 21, 22, 183.

side of the same street, in adjoining houses, in other rooms of the same building, or other corners of the sickly room, to account for the difference of results. Everything in this bespeaks the existence of a separate morbific agent, penetrating, in combination with the atmospheric air to, or generated in the infected spot, and sparing, owing to reasons I need not stop to explain, neighbouring or adjoining localities.

Some forms of malarial fevers resemble diseases produced by putrid substances introduced into the circulation.—The incorrectness of the views of those who refuse to acknowledge the existence and agency of a malarial poison, will the more forcibly strike us, when, reverting to the preceding circumstances, we bear in mind the great analogy existing between the phenomena of some forms of fevers and those produced by the introduction of putrid substances in the torrent of the circulation, or the forced inhalation of mephitic air, as shown by the well-known and oft-mentioned experiments of Gaspard, Magendie,[1] Leuret, Hamont,[2] and others; by the ingestion of various mineral and vegetable, and the absorption of some animal, poisons[3]—to say nothing of the discovery by Lassaigne, in his analysis of putrid meat water, of a stinking volatile oil, the probable poisonous agent in the effects obtained by those experimentalists, and which can differ but little from the substance dissolved in the air of malarious localities.

Take yellow fever as an example. The late Dr. Harrison, of New Orleans, who was perfectly competent to form a correct opinion on the subject, speaking of the effects obtained from injecting putrid meat water into the veins of animals, as shown in the experiments of Gaspard, already alluded to, says: "No one can, I think, fail to be struck with the extraordinary resemblance of those symptoms and *post-mortem* lesions to those of yellow fever. The characteristics of the disease, its rapid course, its hemorrhagic tendency,

[1] Journal de Physiologie, ii. 1, and iii. 81–85.

[2] Journal des Progrès des Sc. Méd. vi. 181.

[3] Fontana, Traité de la Vipère, i. 85; Celle, Hygiène des Pays Chauds, 89; Sauvages, Nosologie, iii. 112, 115; E. Miller, Works, 52, 53; Med. Repos. ii. 412; Waring, Yel. Fev. of Savannah, 37, 38; John K. Mitchell, Cryptogamic Origin of Fever, 73; Hunter, Dis. of the Army in Jamaica, 156; Dewitt on Stramonium, Med. Reposit. ii. 30; Ferguson, Recollections, 204, 5; Caillot, Fièvre J. 296; Levacher, 78; Chaussier, Consultations de Méd. Légale, 40; Rochoux, 79; Salva, Segundo ano del Real Estudio, &c. 142; Lafuente, Observaciones Sobre la Fiebre amarilla, 201.

its peculiar lesions, are all to be met with in these experiments. We have black vomit, bloody alvine discharges, redness of conjunctiva, extreme tenderness over the abdomen, great and rapid prostration of strength, burning thirst, anorexia, &c.—all so characteristic of yellow fever. In his other experiments, he speaks of other characteristic symptoms—suppression of urine, intussusception of the intestines, the existence of fetid fuliginous matter in the bowels, ecchymosis of the mucous membranes, congestion of the lungs, &c. In short, there is hardly any symptom mentioned by authors, as occurring in yellow fever, which may not be found in these experiments; and it is the same with regard to the *post-mortem* lesions."[1] Let it not be supposed that other substances, when injected into the veins, will produce similar effects. Gaspard, and after him Magendie, have found that many of them cause death, but none give rise to the symptoms or anatomical lesions described above. Those produced by ammonia come nearest to them.

Let us also bear in mind that dead animal matter is found to run faster into putrefaction, in situations where, from the prevalence of fever, marshy exhalations may be supposed to abound; that beer, wine, and other fermentable fluids, if kept there on the ground, spoil sooner than in healthy localities;[2] that the active operation of such exhalations on sores and wounds is often evinced during life; that substances fabricated of silk, wool, cotton, and flax, very rapidly undergo decay when exposed to the atmosphere of such situations—silk and woollen becoming putrid, and cotton and linen assuming a dingy or yellow hue, and afterwards losing their cohesion; and that these effects, which in Italy, France, and other countries are generally recognized as indicative of the insalubrity of particular places and seasons, though rapid and complete in proportion to the moisture and warmth of the air, exhibit themselves to the fullest extent when these conditions are combined with sources of concentrated malarial exhalations.

When, besides, we recollect that the absorption of softened tubercular matter, in small proportion, and in the early stage of its formation, produces an intermittent form of fever, of quotidian type, and recurring in the evening; that, with the progress of the softening of this matter and of its absorption, the type of the fever

[1] Speculations on the Cause of the Yellow Fever, N. O. J. iii. 570.

[2] Barton, on Hong-Kong Fever, Dublin J. (N. S.) xii. 345.

changes, becoming first remittent, and, finally, continued; when we recollect that the fever attendant on the absorption of pus formed in the parenchyma of the liver often assumes the tertian type, and that intermittent febrile paroxyms of great regularity have resulted from such absorption;[1] when, finally, we bear in mind the changes which take place in the system of those who recover from yellow or other malarial fevers, through the unaided efforts of nature; the coincidence of the sudden and successive disappearance of the symptoms with the appearance of phenomena of a critical character—dark discharges from the bowels, abundant sweats, and a copious flow of flocculent and sedimentous urine—so like what is known to attend the expulsion or elimination of some poisonous substance, it is impossible to avoid the conclusion that fevers, when they make their appearance in the localities mentioned, are the offspring, not of a cause of a general character or of physical influences, consisting of some particular modification in the ordinary and sensible qualities—thermometrical, hygrometrical, electrical, &c., of the atmosphere; since these operate in equal degree all around, without, however, producing everywhere the morbific effects in question; but of something exhaled from the soil, or from some of the various sources of decomposition adverted to. In other words, we must infer that fevers arise from the impress of a species of morbid material poison, which finds admission in the blood, and occasions peculiar changes in certain of the constituents of that fluid—conferring subsequent immunity in many cases, like other causes of a kindred nature, though in a less degree, and with less certainty; possessing, to some extent, other characteristic properties of true morbid poisons, but differing from them in not being the product of operations taking place in the living system; in its not being possessed of the power of reproducing or multiplying itself in the body; in its not converting any of the elements of the blood into its own similitude, and in its effect not being capable of propagation from one person to another.[2]

The geological formation of sickly localities, and the plants growing therein, &c., lead to a belief in the malarial doctrine.—The conclusion will appear still more natural, when we take the following facts

[1] Boudin, 128, 129; Griffin, Lond. Med. Gaz.

[2] See Simon's Lect. on General Pathology, 198, Am. ed.

into consideration: Malaria is more rife in localities characterized by particular geological formations. In both the West and East Indies, places where the mangrove and mancilina grow luxuriantly, the most unhealthy are those in which the roots of those plants are only occasionally under water. In general, in tropical latitudes, the existence of a large quantity of astringent plants—the bark of which contains a large portion of animal matter, combined with tannin—is connected with the development of fever.[1] Mr. Boudin informs us, in his *Treatise on Periodic Fever*, that, from some experiments made by himself, he is inclined to the opinion that certain plants, more than others, have the power of giving rise, by their decomposition, to the evolvement of febrific exhalations. Such are some of the *algæ*, as, for example, the *chara vulgaris*. He attributes the same febrific faculty to the *rizophore* and the *calamus* (p. 59). In some parts of France (the Department of Ain—see the statistics of that Department, p. 206), a similar agency is attributed to the *Anthoxentum odoratum*.

Cause more effective near the surface of the earth than at a distance.—The cause, though sometimes wafted to elevated spots, generally manifests a tendency to remain near the surface of the soil, attacking more generally, and with greater malignancy, individuals occupying the lower floors of houses.[2] In Italy, and along the shores of the Mediterranean, as well as in the West Indies, the ground floors are never occupied by those who can avoid them; and, in some parts of the latter country, the houses are built upon pillars, by way of avoiding the poison. When the Corsican peasants are obliged, in the autumn, to leave their hill-towns, for the purpose of working in the fields below, they never fail, if possible, to return home in the evening. When, however, the distance is too great, they construct temporary cabins on the tops of the trees, to which they carefully ascend for the night.[3]

Much of this may arise from the refrigeration produced by upward radiation of heat, which, as is well known, exercises a marked

[1] Humboldt, 763–771; *ib.* Personal Nar. iii. 191, 372.

[2] Smith, Fev. of Gibraltar, Edinb. J. xxxv. 35; J. Hunter, 306; Cycl. Pract. Med. ii. 280; Ralph. Trans. of Edinb. Med.-Chir. Soc. ii. 57, 59; *ib.* Rep. in Ferguson, Med.-Chir. Tr. viii. 170; Bancroft, Seq. 448; Blair (notes), 30; Caldwell on Malaria, 130; Second Rep. on Quar. 60.

[3] Jacquot, 9.

influence as an exciting cause of disease.[1] But this radiation, however powerful in the latter capacity, and however likely to arouse into action the dormant efficient cause, cannot of itself give rise to any of the various forms of autumnal fevers, seeing that it exercises the influence under consideration only in localities the infectious character of which is well ascertained, and where febrile attacks take place without its agency. Add to this that diseases justly referable to its sole agency belong to a different class.

Cause of fevers destroyed or mitigated by sanative measures.—The morbid effect of the cause is destroyed or mitigated, and its action neutralized by disinfectants and antiseptics, which operate in the same way on contagious poisons, and arrest the process of decomposition in animal and vegetable substances. The beneficial effects of these agents, in all the cases in question, indicate a similarity of nature in the morbid causes upon which they exercise their neutralizing and destroying influence. Cold, a most powerful disinfectant and antiseptic, which prevents decomposition for an indefinite, if not infinite, period, arrests, as we have seen, the prevalence of fever. Of the disinfecting or deodorizing power of fire or heat, much has been said by philosophers and physicians in olden times; indeed, if the claims of any hygienic means to our respect could be enhanced by its antiquity, few would be more entitled to it than the one in question. It was recorded long before the days of Pliny, who speaks of it as of a thing well known: "There exists in the very fires a remedial power against pestilence occasioned by obscuration of the sun by clouds, and by an excess of moisture. Fire, by its fumigation, certainly assists in many ways. Empedocles and Hippocrates have demonstrated this amply."[2] Poets, too, recognized the benefits derived from that agency. "Or all noxious principles," Virgil says, "may be dried out of them by fire, and useless moisture driven out:—

> 'Sive illis omne per ignent,
> Excoquitur vitium, atque exsudat inutilis humor.' "

The reader will doubtless recollect that it is reported of the father of medicine, that he changed the morbific state of the atmosphere

[1] See on this subject an excellent Report in the 6th vol. of Trans. of Am. Med. Assoc.

[2] Cap. 27, lib. 36.

at Athens, during the plague described by Thucydides, by kindling large fires.[1] The same is related of Acron of Agrigentum.[2] At a less remote period, the city of Rome furnishes a striking example of the benefit which was supposed to arise from the same practice. "Rome," as Lancisi remarks, "an unwholesome region may, thanks to her furnaces, be inhabited with safety." Monfalcon informs us that, when the French troops occupied the Mantuan, during the early Italian campaigns of Bonaparte, they were forced to encamp on the marshy surfaces which abound in that province, and in consequence exposed to malarial fevers. Bonaparte succeeded in preserving the health of his men, by ordering them to keep, day and night, near large fires, kindled for that purpose.[3]

Sir Gilbert Blane speaks in the highest terms of heat as a purifying agent on board of ships, and states that nothing served to contribute so much to disinfect the filthy French ships captured by Admiral Rodney in the famous battle of 1782, and sweeten the air in them, as burning fires in the hold.[4] And we all know the advantage resulting, in all malarious localities, from warming and drying the houses, especially early in the morning, and at the approach of night.

Chlorine, and other fumigations, destroy ammonia and organic bodies with more or less facility; and there are not wanting facts to show that they exercise a salutary influence in mitigating or arresting the progress of fever.[5] The same may be said of chloride of lime, whose efficacy, though doubted by some observers,[6] is highly thought of by reliable professional authorities in this and other countries;[7] of chloride of zinc, which is highly extolled by competent judges;[8] of smoke, which has been successfully employed

[1] Galen, Therap. ad Pison; Aëtius, v. 94.

[2] Plutarch, De Iside et Osiride, see Adams's Trans. of Paulus Ægineta, i. 274; *ib.* Tr. of Hippocrates, i. 12.

[3] Traité des Marais, 201.

[4] Diseases of Seamen, 117, 287; see also Blane's Dissertation, i. 220; Folchi, N. A. Med. and Surg. Journ. vii. 252; J. Clark, 67.

[5] Savaresi, 451; Dariste, 227; Bally, 591; Valentin, 233; Arnold, 18; Robert, 574; Townsend, 223; Cullom, 365; Guyton Morveaux, Carmichael Smith, Playfair, Graham, Hoffman, Cooper, in Sec. Rept. of London Commissioners, 1848, p. 32, &c.

[6] Bowie, in same work, 83.

[7] Johnson, Some Account of the Origin and Prevalence of Yellow Fever in Charleston, Charleston Journ. iv. 164; Southern Agriculturist, iv. 250, 417, as quoted by Dr. Johnson.

[8] Bryson, 225.

in Germany, France, and elsewhere, on land and on shipboard;[1] as well as of the sprinkling of lime,[2] and of ozone, the most powerful of disinfectants.[3]

Dr. Stokes relates somewhere the instance of a district of Cornwall, where paludal fevers prevailed extensively, and have disappeared since the establishment of a copper foundry. It is known that during the fusion of copper, an escape of arsenical particles takes place. In this respect malarial diseases approximate to those produced by animal poisons. M. Bousquet has found that the

[1] Zimmerman, de l'Expérience, notes by the translator. Bonnet, Essai sur la Purification de l'Air, 17; see also Hoffman and Van Swieten. Galeron, Mém. de la Soc. Roy. de Méd. iii. 44; Lind on Seamen, 74.

[2] Forbes's Review, July, 1844, p. 196.

[3] This substance, which at one time was supposed to constitute the efficient cause of epidemic diseases, because it is sometimes found in the atmosphere during the prevalence of these, may now, if the experiments of Dr. Schœbein[1] are to be relied upon—and, so far, no doubts have been expressed on the subject—be considered as the most powerful disinfectant, and the great purifier of the atmosphere. It destroys, quietly and effectually, the miasma disengaged from putrid flesh; and there is every reason to believe that it acts as efficiently in regard to the cause of fever, as to atmospheric poisons, artificially produced; whilst its effects in respect to these and its virtues as a destroyer of the cause in question, lend a strong support to the opinion which ascribes fever to the existence, in the air, of peculiar poisonous exhalations. Ozone is abundantly found during thunderstorms, and we know that these purify the atmosphere, mitigating or arresting the spread of epidemic fever. If it is diminished in volume, by the presence of impurities artificially produced, and whose presence in the atmosphere cannot be doubted, it is, in like manner, found in less quantity in hot seasons and fever regions, where malaria may be supposed to exist in greater abundance; and if, in the former case, the diminution in question is produced by the action of the disinfectant, in neutralizing or destroying the existing impurity, we cannot greatly err in referring the diminution, in the latter case, to a like action of that substance on a kindred poison, and its consequent consumption. Ozone, on the other hand, exists in greater abundance in winter; and, as this is precisely the season at which miasmal fevers do not prevail, and when the atmosphere is in the greatest state of purity, we are justifiable in the conclusion that its accumulation, at that period, arises from a less demand of it for the decomposition of oxidable miasmatic matter or poison. Again, it has been found that the higher stratas of the atmosphere are more ozoniferous than the lower ones, an effect easily accounted for by the circumstance that those strata contain a less quantity of that oxidable miasmatic matter than those portions of the atmosphere which are nearer the surface of the earth, and that hence a smaller quantity of the disinfectant substance is consumed. In a word, ozone acts like chlorine, by destroying impurities existing in the atmosphere; and, if it purifies the air of infected localities, and destroys the cause of fever, it can only do so by destroying or neutralizing a kindred impurity, or poisonous exhalation, floating in the atmosphere of such localities.

[1] Med.-Chir. Trans. xxxiv. 212.

chlorides produce an important change in the vaccine matter, and, indeed, annihilate its specific property, when the mixture is properly made and somewhat prolonged.[1] The syphilitic virus, according to Ricord, unless not very much diluted, is not affected by mixture with saliva, urine, vaginal mucus, muco-purulent matter of the urethra or vagina, fecal matter, sweat, or sperm ; but is destroyed by an alkali, or an acid, as the sulphuric, nitric, chlorhydric, acetic, &c.; by chlorides, potash, soda, ammonia, wine, alcohol, concentrated decoction of tan.[2] Vaccine matter is destroyed by great heat,[3] and by frost.[4]

Cause arrested by trees, &c.—The extension or diffusion of the cause is arrested by trees, walls, hills, rows of buildings, canvas, and other such obstacles.[5] Lancisi cites a number of facts showing the advantages of belts of trees in protecting against the effects of malaria, and the danger resulting from their removal. He calls attention to the fact that, in former days, there existed on the south side of Rome a thick forest. It extended from Frascati and Albano to the Tiber, and protected the southern portion of the city, and the neighbouring district, from the baneful influence of the effluvia of the Pontine Marshes. This rampart has since been removed, and the country has become proverbial for its unhealthiness.

Lancisi did not for a moment doubt the utility of these belts, and expresses the opinion that the consecration by the ancients of woods and groves had no other motive than guarding, through their means, against the diffusion of the febriferous poison.[6] In this, he was probably right. Among the Romans, the advantage of such barriers had long been recognized. Trees were planted in rows and in masses, to guard against the diffusion of malaria. The practice was enforced by law, and recorded on the Roman tablets. This law, which was reported by Cicero—"Lucos in agris habinto"—

[1] Nouveaux Traité de la Vaccine, 226.

[2] Tr. Prat. des Malad. Vénér, 178; Lettres J. 86.

[3] Anglada, i. 213.

[4] Bulletin, ii. 1051.

[5] Rigault de l'Isle, in Johnson, Trop. Cl. 307, 314, 316, 317; Mitchell (J. K.). 26, 100; Macculloch, 116, 252, 3; Williams, Morbid Poisons, ii. 448, 453; Johnson, Change of Air, 143; Evans, 15; Caldwell on Malaria, 135; Tournon, *loc. cit.* i. 209; Watson's Practice, 453; Bonnet, Fièv. Interm. 311; Drake, i. 727; Monfalcon, 93, 94, 160; Annesley on Diseases of India; Wilson Philip, on Fevers, i. 79; Hosack, Practice, 174; Dundas, Sketches of Brazil, 246; De Renzi Miasmi Paludosi, 30.

[6] *Op. cit.* 89, &c.

had reference evidently much more to the advantage in question, than to the purposes for which trees are usually planted. In order to insure their safety, such collections of trees were placed under the protection of some divinity, or under the responsibility of the Roman consuls.

"Sive sacro pavi, sedive sub arbori sacra," says Ovid.

"Si canimus sylvas, sylvæ sint consula dignæ," according to Suetonius.

Bapt. Donus, in his work on the means of insuring salubrity to the soil of the Roman States, recommends the planting of pine and other trees between Rome and the Pontine Marshes, to intercept the miasmata wafted from these by the south-west winds.[1] At Velletri, as also at Campo Salino, the destruction of belts of woods was followed by the prevalence of fever.

The following fact, published by Dr. Lewis in his medical history of Alabama, on the authority of Dr. Wooten, at present, I think, Professor in the Medical School of Memphis, is interesting and apposite: "Mr. P. E. had negro-quarters situated on the first prairie elevation above the low grounds of a small creek, the fourth of a mile from the houses. The belt of low ground frequently overflowed, causing water to remain in holes over its entire breadth, on the subsidence of the stream, but it was well shaded by a dense foliage, the plantation lying on the prairie in the rear of the cabins. In the winters of 1842 and 1843, the trees between the houses and creek were cleared away; and up to that time, some eight or ten years, the negroes living in this quarter had enjoyed uninterrupted health, a case of fever scarcely ever occurring. During the summer of 1843, the first after the forest had been cleared away, fever prevailed among the negroes with great violence, continuing until frost. The negro-quarters were afterwards removed to the opposite side of the creek, about the same distance from it, but with an intervening growth of timber, and no fever has occurred on the place since."[2]

"Whole families," says Mr. Bartlett, "have resided near the Pontine Marshes, and, by the intervention of shrubs and trees, have escaped for years the noxious effects of the mephitic vapours which those putrid waters engender."[3] Dr. Hosack states that a family in

[1] De Restituenda Salubritate Agri Romani, 1667.

[2] New Orleans Journal, iv. 4.

[3] Thompson's Annals.

New Jersey was attacked with fever in consequence of cutting down a wood that separated them from a morass in the neighbourhood. Before that operation, they had been healthy.[1] "Army physicians, therefore, recommend," says Dr. Wilson Philip, "having a wood, if possible, between marshy grounds and an encampment."[2] Rigault de Lisle calls attention to the fact that, upon Mount Argental, above the village of St. Stephano, there is a convent, which has lost all the reputation for salubrity which it once enjoyed, since the lofty trees, by which it was surrounded, have been cut down. I have been informed, he adds, by persons worthy of credit, that, in consequence of the felling of the wood before Asterna, near the Pontine Marshes, Velctri was visited for three successive years by diseases which made much greater havoc than usual throughout the whole country, and penetrated to many places which they had not previously been accustomed to reach. Rigault de Lisle cites other cases, and refers to Volney, who states that Beyroot, formerly very unhealthy, has ceased to be so since the Emir Fakr-el-din planted a wood of fir-trees, which still exists, a league below the town.

By Pliny and others, among the ancients, it was supposed that trees absorb the exhalations extricated from insalubrious places, and that the beneficial effects obtained from woods are to be accounted for in this way much more than by the obstacles they offer to the diffusion of those exhalations. This opinion has, to a certain extent, received the sanction of Thouvenelle, Copland, and other modern writers; and its correctness is rendered probable by the results of certain experiments made long ago, and repeated more recently to ascertain the fact. "Plants," says Julia de Fontenelle, "which Priestley had inclosed in glass jars filled with vitiated air, continued to thrive, and, at the end of a few days, this air had become as pure as that of the surrounding atmosphere."[3] A more recent writer, Dr. Lewis, of Mobile, reverting to the subject, remarks: "It is the generally received opinion that living vegetation protects the human system from the deleterious effects of malaria; and, reasoning by analogy, it would appear that experiments made by scientific men have satisfactorily explained the mutual dependence of the animal kingdoms on each other for support. It has been ascertained that if air, rendered pernicious by respiration, be

[1] Practice of Medicine, 174.
[2] A Treatise on Feb. Dis. i. 79. Am. ed.
[3] *Op. cit.* 139.

confined in a bottle, into which some green plant has been introduced, and exposed to the action of the sun, the carbonic acid will be absorbed, and the air restored to its original condition. The putrefaction of animal matter, and the decomposition of vegetable substances would cause a sufficiency of carbonic acid vapour, when united with atmospheric air, to destroy every living being, were it not for this wise provision of nature. This gas, which is poisonous to the human as well as animal species, is a source of nutriment to every variety of plant; and thus, it would appear, exercises a benign influence in protecting men from the deleterious effects of poisonous vapours."[1] And if the effect is obtained so far as regards one species of poisonous vapour, it may be equally so in reference to that giving rise to fever.

Facts, indeed, are not wanting to show that aquatic plants, as well as certain vegetable substances, growing in damp and swampy or marshy soils, possess the property of disinfecting them—a virtue which, as is well known, Dr. Cartwright ascribes more particularly to the *Jussiena Grandifolia*, an exclusively aquatic plant found in great abundance in some parts of the Southern States, and especially in some regions of Louisiana, which, though presenting many of the usual characteristics of malarial surfaces, are exempt from fever.[2]

A distinguished natural philosopher, Changeux, inferred from the results of his experiments, that the action of trees in the production of the effect under consideration is twofold. "Plants," he says, "whether odoriferous or inodoriferous, give issue to emanations, which, when mixed with poisonous vapours exhaling from marshy or damp soils, neutralize their pernicious influence. But the former exercise a greater effect through means of the neutralizing process than by the power of absorption just mentioned; their emanations mixing with the air we breathe, and correcting its deleterious properties by virtue of the particular qualities with which they are endowed. The second class—the inodoriferous, on the other hand, act more evidently through means of their power of absorption than of the neutralizing property of their emanation, and remove from the air the vapours by which it is contaminated."[3]

[1] Med. Hist. of Alabama, New Orleans Journ. iv. 4, 5.
[2] Western Journal of Medicine and Surgery, i. 428, &c.
[3] Journal de Physique, vi. 211.

By not a few able observers and expert experimentalists, the disinfection is ascribed, not to the absorption by trees and other vegetable substances of the gaseous poison floating in the atmosphere of malarial localities, but to the purification of such an atmosphere through means of the large supply of oxygen obtained from living plants, and the neutralizing agency of that gas on the mephitic particles it meets with in insalubrious places.[1] As to the manner in which the oxygen thus produced destroys or prevents the elaboration of the malarial poison, some difference of opinion exists. M. Carrière, in his excellent work on the climate of Italy, adopts the views of Chevreul and Fontana, in relation to the formation of the febrific poison through means of the action of organic matter on the sulphates contained in the earth or in water with the aid of the oxygen derived from the former. According to this writer, the leaves of plants and of trees, as well as the green substances that cover the soil, are all inexhaustible sources of oxygen, which is so important to sustain life and preserve health. This fluid, thus furnished, offers an obstacle to the action of organic matter. If the latter acts chemically on the sulphates, the other, in its turn, reacts on those compounds, and, from the double antagonistic action thus produced, a state of equilibrium, advantageous to the purity of the air and the salubrity of the country, is re-established. "Hence, to cover the fields, the edges of marshes, and the whole extent of the soil with an abundant vegetation, is equal to placing on the surface of unhealthy regions a reparative apparatus of the greatest power." "Trees, therefore, must have a large share in the amelioration of the country, in consequence of the quantity of leaves they furnish."[2]

By others, again, it has been remarked, doubtless with much truth, that malaria is collected by plants, particularly those of a dense and entangling foliage, so as to be disengaged on cutting them down or rooting them up, thus exciting fever in the labourers who might otherwise have escaped, as proved by the circumstance that in all those situations while the workmen are in the erect posture, and engaged at their work, they escape the fever, but are attacked if they sit, and, more particularly, if they lie down on the ground—and that whether they sleep or not.[3] Whether these views

[1] Senebier, Physiologie Végétale, iii. 184, &c.

[2] Le Climat de l'Italie, 328.

[3] Edin. Rev. xxxvi. 546.

be correct or otherwise; or whether the effect is generally due to the mechanical obstruction trees afford to the transit of malaria from its source, while the exemption of individuals who keep the erect posture, and their liability when they sit or lie on the ground, are to be explained by a difference in the amount of terrestrial radiation, or by the greater quantity of contaminated air they breathe, I need not stop to inquire, and dismiss the subject with the simple remark that, the same results being obtained from the interception occasioned by walls, houses, hills, &c., we are justified in inferring that trees, though they may perhaps act as absorbents, act also, and principally, in the mechanical way mentioned. Be this as it may, Dr. Ferguson, calling attention to the attraction of marsh poison for, or rather its adherence to, lofty umbrageous trees, says that "this is so much the case that it can with difficulty be separated from them; and that in the territory of Guiana, particularly, where these trees abound, it is wonderful to see how near to leeward of the most pestiferous marshes the settlers, provided they have this security, will venture—and that with comparative impunity—to place their habitations. The town of New Amsterdam, Berbice, situated within musket-shot to leeward of a swamp extremely offensive at a certain stage of dryness, owes, evidently, its ordinary exemption from fever to this cause." "A still better instance of the same, and with the same results, may be seen at Paramaribo, the capital of Surinam, where the trade-wind, that regularly ventilates the town, and renders it habitable, blows over a swamp within a mile of the town, which, fortunately for the inhabitants, is covered with the same description of trees."[1] But whatever be the way in which these operate in promoting the salubrity of malarial localities, the effect tends to show that the benefit obtained is due to the destruction of a poisonous agent floating in the air, or to the obstacle it encounters in its passage with the atmosphere from one place to another.

Fever arrested by removing sources of infection.—The progress of fever has been arrested by the clearing and washing of gutters, streets, sinks, and sewers—by the removal of other sources of effluvia; while in ships, as we have already seen, epidemic prevalences of yellow fever have been arrested, and health restored, by kindred means.

[1] Marsh Poison, in vol. of Notes and Recol. 195, 6. See also Williams on Morbid Poisons, ii. 448.

Fever sometimes connected with the existence of certain fogs or mists.—Instances are on record in which the surfaces of highly insalubrious marshy localities have been found at particular hours of the day, covered over with a heavy foggy cloud of a peculiar character, and which carries infection[1] wherever it is propelled; while ordinary fogs in the vicinity, or elsewhere, are perfectly innocuous, so far as relates to the production of autumnal or periodic fevers. Such was the case, for example, in Wilmington, in 1798. Such also has been found over the town of Huaura, in South America, where marshes are extensive. There malaria is stated to have been found distinctly separate from the atmosphere, lying at an average of two or two and a half feet above the marsh, and distinguished by a peculiar kind of opalization, which, on certain changes of light, exhibited a yellowish tint.[2]

Allusion has already been made to the smokes of the African coast. These very frequently, if not generally, carry with them the seeds or cause of fever, and, while doing so, impress the system in a different way from ordinary fogs. In a case mentioned by Sir J. Pringle on the authority of Mr. Lauder, surgeon of a horse regiment, the meadows and marshes on each side of the road which the troops had to pass over on their way to forage, were covered, at an early hour, with a thick fog of an offensive smell, which he considered as the chief cause of the sickness which affected the men, as few who were exposed to its impression escaped an attack.[3] Pringle also states that, in 1748, the nocturnal fogs near the inundations, were thick and fetid.[4] Dr. J. Johnson states that when, at Batavia, the land-breeze came off from the low swampy grounds about the place, early in the mornings, it brought with it a thick mist, accompanied by a very fetid smell; all of which would gradually go off as the sun rose, and the sea-breezes set in. During the prevalence of this fetid mist,, many people complained of slight indisposition in the head and stomach, which, likewise, went off as the sun rose.[5] We know, however, that all do not get well who breathe that mist, for the great prevalence of fever about Batavia is proverbial.

The following case occurred in St. Lucia: Two men, after finish-

[1] Vaughan's Med. Rep. iii. 36.
[2] Von Tschudi's Travels in Peru. See Edinb. Journ. lxix. 489. Watson, 451.
[3] P. 178. [4] P. 63.
[5] Trop. Climates, 127.

ing their day's work, before returning home, were occupied in hauling their canoe high upon the beach, close to the most dangerous part of a large swamp, when they perceived, immediately to windward, a small cloud of vapour gradually approaching them. In a short time they were enveloped in it. One of them fell down, apparently in a state of asphyxia, and the other was so affected as to be unable to render him any assistance. The latter soon recovered, while the other, after coming to sufficiently to be led home, was seized in the night with an intense ague, during which the surface of the body was cold, the countenance expressed great anxiety, and the pulse was small and scarcely perceptible. The patient was insensible to surrounding objects, and in a state of coma, only interrupted by severe convulsions. This cold stage continued about three hours, and was followed by reaction attended with coma alternating with delirium, vomiting of mucosities, and pain in the stomach aggravated by pressure at the epigastrium. A remission followed, which, in its turn, was succeeded by another paroxysm equal in violence to the last, except that the cold stage was scarcely perceptible. Death occurred about forty hours from the period of exposure. On dissection, the blood was found fluid, and a small quantity of turbid serum was effused between the arachnoid and pia mater; the lungs were somewhat engorged, and the stomach was intensely inflamed, containing two or three ounces of blood in its cavity. The other man stated that the vapour had no perceptible smell; that it was warm and moist, like steam, stopped the respiration for a moment, and produced a sense of faintness and trembling of the whole body.[1]

The stagnant water of marshes injurious to health.—Stagnant water, particularly that contained in marshes and swamps, cannot support animal life, and furnishes no element for the sustenance of fish. The discharge of such water into ponds or lakes has not unfrequently given rise to the sudden destruction of those animals contained therein.[2] To produce such an effect, it must contain in solution some poisonous matter; and if so, it is not unreasonable to suppose that this poison is taken up with the vapour absorbed in the atmosphere, and that it affects those who are exposed to the influence of the latter.

[1] Evans, R., Clinical Treatise on the Epidemic Fevers of the West Indies, 21.
[2] Monfalcon, 44.

Nor is it unreasonable to admit that the water of paludal surfaces—of marshes, particularly—may, when used as drink, give rise to diseases similar to those resulting usually from the action of the exhalations evolved from them. Centuries ago, Hippocrates, in his masterly work on *Airs, Waters, and Places*, while treating in a special and interesting section of the nature and morbid effects of such water, attributes to its use the enlarged spleen so frequently noticed among the inhabitants of marshy districts—a pathological condition which we now view as one of the effects of malarial poisoning; and though there can be no doubt that this deleterious effect is usually the result of the absorption of the exhalation in a gaseous form through the agency of the respiratory process, there are facts to show that it may sometimes arise from the internal use of water impregnated with the poison. M. Boudin, in support of this opinion relates the following case: A Sardinian vessel, the Argo, left Bone (Algeria) in July, 1834, with 120 soldiers in good health. During the passage to the lazaretto of Marseilles, thirteen of these soldiers died, and at the arrival of the vessel ninety-eight had the fevers, of all forms and types. While such was the case among the soldiers, the crew remained in health. On inquiry, it was found that this result arose from the fact that the crew had made use of pure water they had procured for that purpose, while the soldiers had been obliged to content themselves with water derived from a marshy locality situate near Bone. The few soldiers who escaped, were those who had purchased water from the Sardinian sailors.[1] It must be added that two other vessels which sailed at the same time from Bone, filled, like the Argo, with soldiers, but in which pure water was used by these as well as by the crew, arrived at Marseilles in good health.[2]

The following fact, related by Dr. Drake, on the authority of Dr. Trowbridge, deserves, on more accounts than one, to be recorded. In the neighbourhood of Buffalo, for three (but not successive) autumns, a local epidemic fever occurred among about twenty families, who drank, or otherwise used water, from the same spring. It burst out beneath a ledge of limestone, about twenty-five feet below the summit, beyond which, at the distance of a mile, there was a piece of woodland with a pond, which Dr. Trowbridge supposed to be the source of the spring; for, after rains, its waters became tur-

[1] Boudin, Fevers Interm. 66, 67. [2] *Ib.* Géograph. Méd. 55.

bid. The autumns in which the fever prevailed were unusually dry. In its symptoms and violence, the disease might have passed for yellow fever. Nine or ten persons died. The surrounding neighbourhood remained healthy. The spring was at length abandoned, and the fever did not return. Dr. Drake remarks that this *seems* to show that the material cause of autumnal fever may be absorbed by water, and thus produce its characteristic effects.[1]

A paludal atmosphere enfeebles health.—The feeble health entailed by a long residence in fenny and fever regions, results often from a frequent repetition of febrile attacks; as, frequently, it manifests itself without the occurrence of open fever—intermittent, remittent, or continuous. It depends on pathological conditions of a special kind, characterized by phenomena equally peculiar, and indicates the agency of a morbific cause other than a mere modification in the relative proportion of the natural constituents of the atmosphere, or in the thermometrical or hygrometrical state of the latter. Enlargement of the abdomen, and engorgement of its viscera, and a morbid state of the blood, consisting in the simultaneous diminution in the proportion of the globules, of the albumen of the serum, and sometimes of the fibrin, attended with a bloated, pale, and sallow countenance, a flabbiness of the flesh, an œdematous condition of the cellular tissue, and, in some localties, a disposition to ulcers and to gangrenous sores, are frequently, if not always encountered in such regions, and have been noted by high professional authorities.[2] The evil which this poison inflicts on man is painful to tell, amounting in time to a total humiliating degeneracy of the race. The late Professor Caldwell remarks, in one of his works,[3] that those who would witness that result, in its highest degree, must visit some of the marshy and sickly districts of Europe, more especially of France, Holland, Italy, Spain, and Portugal; for in some of them, where, by the operation of the poison,

[1] Diseases of the Mississippi Valley, 382.

[2] Fodéré, Méd. Lég. v. 165; Nepple, Fièvres Intermit. 11; Boudin, Fièvres Inter. 188; Dict. des Sc. Méd., article Marais, 533; Monfalcon, viii. 114, 131; Blair, Y. Fev. of Demerara, 23; Davy, Notes to Blair; Prony, Rapport sur les Marais Pontines, Becquerel, 170, 1; Edinb. Med. and Surg. Journ. lxxx. 212; Thouvenelle, Climat de l'Italie, 15, 16; Copland, art. Endemic Influences, i. 761, Am. ed.; Carrière, *op. cit.* 87; De Renzi, Miasmi Paludosi, 83.

[3] Essay on Malaria, 19.

through a long and unbroken line of generations, the effect has reached its maximum, the issue is deplorable. The adage, which attributed the dulness for which the Bœotians were proverbial to the mists and paludal nature of their ill-favoured country, is as old as the time of the ancient Athenians, and shows that, at an early period, the cachectic effect of such localities was matter of observation. Hippocrates may have had them in his mind, when, in writing the famous treatise so often referred to, he remarked, that such waters "as are marshy and stagnant, and belong to lakes, are necessarily hot in summer, thick, and have a strong smell, since they have no current; but being constantly supplied by rain water, and the sun heating them, they necessarily want their proper colour, are unwholesome, and form bile. Those who drink them have large and obstructed spleens, their bellies are hard, emaciated, and hot; and their shoulders, collar-bones, and faces are emaciated; *for* their flesh is melted down and taken up by the spleen, and hence they are slender; such persons, then, are voracious and thirsty; their bellies are very dry, both above and below. They are very subject to dropsies of a most fatal character; and, in summer, dysenteries, diarrhœa, and protracted quartan fevers frequently seize them; and these diseases, when prolonged, dispose such constitutions to dropsies, and thus prove fatal. Women are subject to œdema and leucophlegmasia. The children are particularly subject to hernia, and adults to varices and ulcers on their legs; so that persons with such constitutions cannot be long lived, but, before the usual period, they fall into a state of premature old age."[1]

——— Quod si
Judicium subtile videndis artibus illud
Ad libros et ad hæc musarum dona vocares
Bœotum in crasso jurares aëre natum.—HORACE, *Epistol.* lib. ii. Ep. 2.

Lancisi did not fail to notice the undermining effects on the mind and body of an atmosphere thus vitiated, and closes a long and interesting chapter on the subject with remarking, "Adeo colore pallidi, vitibus habetes, ingenio sunt tardo, muliebrique;"[2] and that matters have not changed in that respect in Italy, since his days, may be easily found on consulting the works of Carrière, De Renzi, Thouvenelle, and others.

M. Bosse has furnished us with a graphic picture of the condi-

[1] Airs, Waters, and Places, Adams's Transl. i. 195, 196. [2] *Op. cit.* lib. i. 70.

tion of the inhabitants of the malarial districts of the Department of Ain (formerly the province of Bresse), in France. "A pale and livid complexion, a dull and heavy eye, swollen eyelids, a wrinkled face, narrow shoulders, a contracted chest, a long neck, a shrill voice, a skin always either dry or bedewed with debilitating sweats, a slow and sluggish walk, &c., characterize such an individual, who is old at thirty, broken and decrepit at forty or fifty."[1]

Not different is it in the malarial districts of our Southern States, for there the result adverted to is already visible. In those districts the human frame is weakly constituted, the mortality of children very great, and the mean duration of life short. "Along the frontier of Florida, and the southern borders of Georgia," says Dr. Forry, "as well as in the low lands of our Southern States generally, may be seen deplorable examples of the physical and perhaps mental deterioration induced by endemic influences. In earliest infancy, the complexion becomes sallow, and the eye assumes a bilious tint. Advancing towards the years of maturity, the growth is arrested, the limbs become attenuated, and the viscera engorged. Boys of fifteen years may be seen bowed down with premature old age—a mere vegetating being, with an obstructed, bloated, and dropsical system, subject to periodic fevers, passive hemorrhages, and those other forms of disease which follow in the train of malaria."[2]

These phenomena, which are indicative of that condition of the system denominated very aptly the paludal cachexia, are certainly not the usual results of the action of simple heat, cold, moisture, or atmospheric vicissitudes, or of the introduction into the system of any known gas; but point to the morbid influence of some special toxicological agent absorbed into the circulation along with the air we breathe, and possessing a twofold action, one chronic another acute; the former producing peculiar bad effects in the blood, in the liver, in the spleen, and the whole alimentary canal and vascular system; the other giving rise to periodic fever.

Dr. Blair remarks that, in Demerara, "there seems to be both an *anæmiating* and a septic malaria. The former pervades the colony, so that a rosy cheek is nowhere to be found after a year's residence; but it least affects the sea-shore. Its intensity increases as we proceed into the interior, up the narrow creeks and muddy

[1] Statistique du Department de l'Ain, 4.
[2] Forry, Climate of the U. S. 365, 366.

rivers. There, the complexion becomes perfectly etiolated, even without an attack of intermittent; the spleen gets enlarged, the cellular tissue infiltrated, and dyspnœa and palpitations supervene from mere whiteness (loss of the red particles) and thinness of blood." (P. 23.) To this Dr. Blair's annotator, Dr. Davy, adds that such an influence seems to be common in the West Indies. "It is strongly marked by the pale, sallow, sickly hue of the white creoles, especially in Barbadoes. There, if they have any colour, it is most frequently reddish; whence the labourers of this class have been called 'red legs.' One rarely witnesses that bronzing of the exposed skin in the West, which is the almost constant effect of exposure to the sun's rays in the East Indies, and in the south of Europe." (*Ib.*)

In many southern regions, the West India Islands, for example, malaria predisposes, as Hippocrates informs us was the case in Greece, to ulcers. Of 6,395 admissions into the colonial hospital of Georgetown, Demerara, 1,873 were cases of ulcers. Small wounds produced by insects and serrated grasses, are sufficient to excite one in subjects so predisposed. The ulcers chiefly affect the lower extremities; but the fingers are sometimes affected, and even the lips and cheeks. In the negroes they are often large, with indurated edges, and produce not the slightest inconvenience, except in the trouble of dressing them. Among the emigrants, particularly the Portuguese and the Coolies, the ulcer is generally sloughy, phagedenic, bleeding, and sometimes a perfect sphacelus, without even surrounding or previous erythema. This last form affects those who have still a tolerably healthy appearance. In time, the soft and solid parts are speedily destroyed, and if amputation be found necessary, gangrene is apt to attack the stump within twenty-four hours. In the anæmiated immigrant Portuguese, the granulations are pale, and the progress of course is slow, but the sore will heal kindly.[1] Dr. Blair is of opinion that this septic modification of the malaria (which shows itself in the bleeding gangrenous ulcer) is some imperfect development of the yellow fever poison. Dr. Davy remarks, in support of this view: "In Barbadoes, among the white natives, who are almost always exempt from yellow fever, the mucous membranes, especially of the primæ viæ, and the skin, are very prone to diseased action of a kind bearing some resem-

[1] Blair, 23.

blance to what is witnessed in yellow fever; for instance, the chapping of the lips, with ulceration and bleeding; an aphthous or slightly ulcerated state of the fauces, and probably of the gullet and stomach; the yellowish sallowness of skin, with tendency to ulceration. I may add that, during the absence of yellow fever amongst the troops, a disposition to purpura hæmorrhagica is occasionally witnessed."[1]

Effect of a paludal atmosphere on the duration of life.—Nor is it to be forgotten that, for the above reason, a paludal atmosphere has the effect of shortening the duration of life. Dr. R. Jackson states, as the result of observations made during the period of our revolutionary war, that white females, born and constantly residing in the lower districts of Georgia, were seldom observed to live beyond the age of forty, and males of fifty. He adds, that he was credibly informed there was not on record an instance of a person born at Petersburg, Va., and constantly residing there, who had lived to the age of twenty-one.[2] Either the distinguished writer was misinformed, or matters have greatly improved in that vicinity; for the average duration of life is infinitely greater now than he represents it to be. But still, at present, the fenny districts of Virginia give but a low average. Dr. James Johnson found that in the valleys of Beveland and Walcheren, the peasants exhibited conspicuous marks of premature old age, and seldom reached beyond fifty-five or sixty years.[3] According to Sausset, the average duration in fenny countries is not over twenty-six years. In many districts of France, it does not exceed twenty-two.

Rozier, quoted by Monfalcon, fixes at fifty years the farthest limit of human life of the inhabitants of Lower Brittany; the old age of whom, when they have attained this point, bears a great resemblance to that of individuals who, in salubrious countries, have reached to ninety.[4] It would be difficult to find a locality where other than a toxicological cause could produce such disastrous results on an entire population. From the researches of M. Fleuriau de Bellevue, we learn that the mortality of some communes around Marenne amounts to one in thirteen, and that that of the Canton of Brouage, during a period of sixteen years—1817–

[1] P. 24, note.

[2] Treatise on the Fevers of Jamaica, 77, 80.

[3] Trop. Cl. 41.

[4] *Op. cit.* 134.

1832, presents a proportion of one to twenty-one.[1] The same writer remarks, in farther illustration of the deleterious effects of paludal localities, in relation to the subject under consideration, that inundated and well-shaded marshes were found to be as healthy as dry and well-cultivated fields—the mortality being one in from 42 to 46; that in districts possessing a purely compact argillaceous soil, unsheltered from the rays of the sun, and on which rain-water remains stagnant for some time, the mortality was one in 25, and even one in 20; and that in five cantons containing several large regular marshes, the mortality amounted to one in 18, and even reached as high as one in 16.[2] In the Bresse and Dombes, the proportion is also one to twenty-one; while in the immediate vicinity, the proportion is much less unfavourable, being from 1 to 25.6 to one to 26.7.[3] In the Department of Loiret (canton of La Ferte and Sully, but not including the town of Sully), the average duration of life is 23.33 years. In the Department of Loir et Cher (cantons of La Motte, Beuvron, Neung, Romorantin, and Salbris), the average is 29.41. In other less malarial cantons, it is 30.04, 30.64, and 34.34.[4]

France, taken in its *ensemble*, exhibits a loss on one in about forty; from which it follows that the number of deaths in the aforesaid malarial districts is twice as large as it is in the country generally, and that the average of life is proportionably less. Among children, the loss has been particularly large, amounting during the first year to thirty-two per cent.; and in some communes to forty-two per cent., or four-tenths (near one-half), while in the entire of France the proportion is only twenty-three or twenty-four per cent.[5]

To the same effect, I may call attention to a fact mentioned by Dr. Price, in a work which has justly acquired a great reputation. Referring to the District of Vaud, in the Canton of Berne (Switzerland), the population of which amounted to one hundred and sixty-nine families, representing a total of six hundred and ninety-six individuals, he remarks that of these, one-half of all born in the mountains live to the age of forty-seven; whereas, one-half of all born in

[1] Statistique de la Charente Inférieur, quoted by Mélier, Mém. de l'Ac. de Méd. xiii. 667.

[2] Comptes de l'Acad. des Sc. xxv. 338, 339.

[3] Fodéré, Médecine Légale, v. 163; Nepple, Fièv. Interm. 11.

[4] Becquerel, Des Climats, 265, 266.

[5] Mém. de l'Acad. de Médecine, xiii. 667.

the marshy portion of the district, reach only to the age of twenty-five. One in twenty, of all born on the hills, live to eighty; only one in fifty-two attain this age in the marshy portion. Hence, he observes, the probabilities of living are highest in the most hilly parts of the district, and lowest in the marshy. In the former, a person aged forty has a chance of eighty to one for living a year. In the marshy localities of the district, his chance is not thirty to one for living a year. In the former, again, persons aged twenty, thirty, and forty, have an even chance for living to forty-one, thirty-three, and twenty-five years respectively; in the latter, on the other hand, persons at these ages, have an even chance of living only thirty, twenty, and fifteen years.[1]

The effects of malaria on population are well illustrated by the results of observations made in the Pontine Marshes, where, notwithstanding the great ameliorations effected in their condition from 1801 to 1811, the mortality has almost always exceeded the births:—

	Localities.				
	Velletri.	Serra.	Poperino.	Sorinoi.	*Total.*
Deaths . .	2,313	3,181	1,717	901	8,112
Births . .	1,786	3,338	1,601	885	7,610

Even in France, where malarial fevers are not as malignant as they are in warmer latitudes, the difference is considerable. Take the ten most malarial departments, and compare them with the ten least so, and it will be found, in 1846, that the former presented a smaller proportion of births and a larger proportion of deaths than the latter:—

	Ten least malarial departments.	*Ten most malarial departments.*
Births	1 in 34.09	1 in 34.40
Deaths	1 in 46.61	1 in 41.08
Increase of population	$\frac{1}{146}$	$\frac{1}{194}$[2]

Cause of fever produces an impress of greater or less strength on all placed under its influence.—The existence of a peculiar morbific agent floating in the atmosphere of malarial localities, and differing from the causes of ordinary diseases, may be inferred from the fact that

[1] Annuities and Lives, ii. 29; *ib.* Letter to Dr. Horsley, on "Insalubrity of Marshy Situations," Philos. Tr. of London, lxiv. 96. See also Sir J. Sinclair, Principles of Hygiene, 84.

[2] Becquerel, Tr. Elem. d'Hyg. Privée et Publique, 193.

in such localities all individuals are more or less, especially in times of severe epidemics, under the influence of the cause, and, without being necessarily ill, give evidence of that influence by presenting in a minor form some of the symptoms of the prevailing disease. By those who have noticed the progress of Asiatic Cholera, and of the wide-spreading Pneumonia Typhoides which traversed this country in 1812, 1813, and who have read of the sweating disease of 1483–85, and of the Black Plague of the Middle Ages, it will be remembered that besides the many who were attacked with those complaints in full force, thousands suffered from some one or more of their characteristic marks in a mitigated form. Phenomena of an analogous kind result from the action of the causes giving rise to remittent and intermittent fevers, yellow fever, and other affections of similar nature; for, during the prevalence of epidemics of these, a large number of individuals exposed suffer from slight indispositions, and exhibit in a variety of ways the impress of the morbific agent. On this subject, the facts recorded by Rush,[1] Mitchell,[2] Potter,[3] Archer,[4] Schnurrer,[5] Boudin,[6] Holmes,[7] Chervin,[8] Pariset,[9] Perlee,[10] and others, leave no doubt; and show the effect to have borne on the eyes, on the secretions, on the alimentary canal, on the brain and nerves, and on the blood. All this is explainable on the supposition of a toxical agent floating in the atmosphere and producing a morbid impress of greater or less force on all, but cannot be accounted for if we refer the disease to the operation of heat, cold, humidity, atmospheric vicissitudes; in a word, to any known modification in the sensible qualities of the air.

Kindred phenomena, resulting from the gradual operation of morbid agents, the true toxical nature of which is undeniable, are matters of frequent observation, and serve to confirm the views here suggested. M. Tanquerel des Planches, in his excellent work on lead diseases, remarks: "Saturnine preparations, when introduced into the system, indicate their presence there before the manifestation of lead diseases by a specific action on most of the solids and fluids." This action, which he denominates primitive introduc-

[1] Vol. iii. 84, 85.
[2] Med. and Philos. Register, iv. 188.
[3] On Contagion, 55.
[4] Med. Recorder, v. 68.
[5] Materiaux pour servir à une doctrine des épidemies, 40.
[6] Fièvres Interm. 188.
[7] Am. J. of Med. Sci. N. S. xii. 308.
[8] Fièvre J. d'Espagne, 174.
[9] Fièvre J. de Barcelone, 27.
[10] Philad. Med. and Physical J. iii. 12, 13.

tion, consists in, 1st, a peculiar discoloration of the teeth, and lining membrane of the mouth; 2d, the saturnine smell, taste, and breath; 3d, the saturnine action, or yellow leaden hue of the countenance; 4th, the remarkable emaciation of the face.[1]

The cause of autumnal fevers produce an impress on the lower order of animals and on vegetables.—The cause giving rise to the febrile affections, which are usually referred to some one or other of the modifications of malaria, extends its influence to the lower orders of the animal creation, and even to vegetables. In this and other cities of the United States epidemics of malignant fever have often been ushered in and accompanied by sickness among cats, dogs, hogs, &c.[2] Similar observations have been made elsewhere—the symptoms being often analogous to those of the reigning disease,[3] while epidemic seasons have often been found marked by a great predominance of insect life. The coincidence of blight with pestilence has been recorded from ancient times. M. Dupuy has seen a number of oxen perish with symptoms perfectly analogous to those of intermittent fever, after having pastured in a highly marshy locality. In 1826, after the overflow of the River Manse, an intermittent epidemic broke out among horses, and occasioned a considerable mortality. Lancisi relates that, in 1713, during the prevalence of intermittent fevers, an epizooty carried off thirty thousand oxen.[4] It should be added, also, that marshes occasion, among many animals, "a chronic endemy, perfectly analogous to that produced in the human species. The prolonged sojourn and pasturage of sheep in marshy localities produce in them the hydroæmia, a disease characterized by a diminution in the proportion of the blood-globules, as also by a notable one in the quantity of the serum, and which,

[1] See also Adams's Rept. in Trans. of Am. Med. Assoc. v. 171, 172.

[2] Med. Repos. i. 250, 254, 5, 351; Smith (E. H.) Fev. of N. Y. 76; Caldwell, 133, 169; *ib.* fever of 1805, pp. 40, 61; Condie and Folwell, 15; Rush, iv. 8; Sheeut, 77; Vaughan, 18; Cartwright, Recorder, ix. 7; Baxter's Med. Repos. xxi. 6, 7; Chapman, Med. and Phy. J. ix. 395, 6; Kilpatrick, N. O. J. ii. 43; Seaman Webster's Collection, 3; Thouvenelle, iv. 200–202.

[3] Smith, Edinb. J. xxxv. 36; Maclean on Epid. i. 289; Sir J. Fellows, 45, 236; Cycl. of Pract. Med. ii. 74; Pariset, 67–69; Osborn, 70; Pinckard, i. 138; Desportes, i. 17; Arejula, 286, 331; Ralph, Ed. Med.-Chir. Tr. ii. 58; O'Halloran, 26; Mélier, *loc. cit.* 669; Rep't on Quarantine, London, 13; Second Rep. 39, 365; Moreau de Jonnes, 112; King's Rep't on Fever of Boa Vista, 9; Blair, 63; Boudin, Fièv. Int. 135; Monfalcon, 502.

[4] Lancisi, De Boville Peste, 2, &c.

consequently, is analogous to the paludal cachexia of men." M. Gasparin reproduced this disease in sheep by causing them to drink and to be rubbed with the condensed vapour obtained from marshy surfaces.[1]

Mr. Chadwick informs us that in the course of the inquiries as to what has been the effect of drainage upon health, one frequent piece of information received has been, that the rural population has not observed the effects on their own health, but they have marked the effects of drainage on the health and improvement of the stock. Thus, the less frequent losses of stock from epidemics are beginning to be perceived as accompanying the benefits of drainage, in addition to those of increased vegetable production.[2]

The General Board of Health of London, in their Report on the practical application of sewer-water and town manures, already referred to, remark that the injurious effects upon health, of the prolonged retention of excessive moisture on a surface of vegetable mould, is established by the production of rot amongst sheep—an effect which sheep-feeders have produced by stocking closes just after they had been flooded, and whilst they were saturated with moisture.[3]

In his examination before the Metropolitan Sanitary Commissioners, already referred to in a former chapter, Mr. Smith, after speaking of the beneficial effects of draining on the healthiness of malarious localities, remarks that it is generally observed by the inhabitants, that their cattle or stock are now less subject to diseases. In a farm in the west of Perthshire, the cattle were very subject to the disease called "red water;" since the draining, there has been no case of that disease. In other parts of Scotland and England, similar results are stated to have followed the introduction of thorough drainage.

Mr. Parker stated that the disease of foothalt in sheep and deer has been perfectly removed in many gentlemen's parks, and in extensive pasturage grounds, by deep under drainage.

"In the Highlands," Mr. Spooner remarked, "and more particularly on the west coast, there exists a well known and fatal disease among sheep, incurable by any treatment, termed 'Braxey,' which on undrained land, and in wet seasons, is a cause of very serious

[1] Becquerel, Hygiène, 183, 194, 195.
[2] Second Rep't of Commissioners of Inq. into the State of Large Towns.
[3] P. 9, London, 1852.

losses. This is, in a great measure, prevented by drainage, and the diminution of casualties alone is more than sufficient to cover its cost, independently of the increased quantity and better quality of the fodder produced. This system has been extensively practised for several years, and invariably with the same beneficial results. As to the health of cattle or stock," he added, "I have the strongest evidence of the beneficial effects of drainage in many instances. On the lands which I possess, and on several others in the district, a disease called 'red water' prevailed, in some years proving very fatal; but after drainage and cultivation of the marshy parts of the pasturage the stock has been free of that disease. The surface drainage of sheepwalks in every district is well known to promote the healthiness of the stock, and I believe the thorough drainage of a single swamp in any locality will be an important means of improving the health, both of the population and stock connected with it."[1]

In farther corroboration of this, it may be remarked that it has been found in this country that those animals that feed in marshes where periodic fevers prevail, have diseased viscera. We are told by Dr. Ludlow, that in the town of Wolcott, Seneca County, N. Y., where marshes and lowlands abound, the hogs, when killed, are generally found to have eroded livers.[2]

The history of the Oriental plague furnishes striking examples of the influence of the morbid cause on the lower order of animals.[3] It has been noticed from the earliest period; is mentioned by poets[4] and historians,[5] and recorded in holy writ.[6]

It is certain, also, "that seasons which are unusually sickly to large classes of human beings, are often alike unfriendly to the health and fruitfulness of many classes of plants."[7] Fodéré, in speaking of the Marshes of Bresse, in France, remarks that animals

[1] Drainage of the Land forming the Sites of Towns, 69, 70. London, 1852.

[2] New York Med. and Phys. Journ. ii. 88.

[3] Hippocrates; Santi Romeo—Ricerche Sulla Peste Borbonica; Diego Picollo, description de la Peste de Mission, chap. 2, 128; Aubert and Etienne, cited by Clot-Bey, 257; Thos. Leslie Greyson, Rep. of Quarantine, 14; Schiller, 257; Short, Chron. Hist. of the Weather and Dis. of Lond. 338; Hancock, 101.

[4] Homer, Iliad, book i. 67, 70; Lucretius, lib. vi. 1117; Ovid, lib. vii. 523.

[5] Thucydides, xi. 52; Livy, 3, 6, 571; Dionysius, lib. xx.

[6] Exodus, vii. 7; xvii. 18; viii. 17; ix. 15.

[7] Report on Quarantine (London), 14; see also Chapman on Epid. Med. and Phys. Journ. vii. 256; Rush, 56; Caldwell, 133; Condie and Folwell, 13; Thouvenel, 200.

and plants are there of a small and feeble complexion, stunted in appearance, and endowed with a small degree of vital force; and Boudin, who refers to this fact, states that he has seen plants which were transplanted in marshy localities deteriorate rapidly, and return to their normal state as soon as they were removed beyond reach of the paludal influence.[1]

The great diffusion and mortality of autumnal fevers is explained only on the supposition of the cause being a gaseous poison.—To no other than a truly toxical agent, suspended in or mixed with the atmospheric air, can we refer diseases which affect in a similar way, and such rapid succession, so large a number of individuals, and occasion often so extensive a mortality. No modification in the sensible qualities of the air of a given locality; no atmospheric vicissitude, however frequent and extensive, has been known to produce such effects. A single regiment, in Africa, with an effective force of seventeen hundred men, sent, in the short space of six weeks, one thousand and fifty to the hospital, and mostly for intermittent and remittent fever. In 1830, the whole of another regiment was sent to the hospital. In the same year (1830), the number of admissions in the Hospital of Buffarick amounted to 2,386; and of these 1,491 were affected with intermittents.[2] According to Dr. Maillot, 22,330 cases were admitted in the Hospital of Bone, from the 16th of April, 1832, to the 16th of March, 1835. Of these, 2,513 died.[3] So common is ague in many parts of Spain and Portugal, says Sir James Macgrigor,[4] "that the inhabitants do not term it a disease." Facts will bear Dr. Williams out in the remark, that the invasion of the Burmese and of the African empires has, in each instance, been so disastrous to the troops, that the whole force must have perished from this class of diseases, in a few months, but for the success of their arms in the one instance, or their being withdrawn from the pestilent atmosphere in the other.[5] The British forces in the Islands of Zealand, amounted, on the 25th of August, 1809, to 41,642 men, and 1,879 commissioned officers. Between the 21st of August and 18th of November, the number of sick, almost exclusively from fever, amounted to 26,846, including relapses.[6]

[1] Fièvres Interm. 198.

[2] Gouraud, Fièv. Interm. 290.

[3] Fièv. Interm. 276.

[4] Med.-Chir. Trans. vi. 415.

[5] Morbid Poisons, ii. 457.

[6] Blane, Dissert. i. 226, 227: see also Marshall, Edin. J. xlviii. 308, 309. The disease began to show itself between the 15th and 20th of August. On the

Dr. Wind, who translated into Dutch Dr. Lind's essay on preserving the health of seamen, and who practised medicine in Walcheren for many years, informs us that the Scotch regiment in the Dutch service has been known to bury its whole numbers at Sluys, in Dutch Flanders, in three years. We learn from the reports of Dr. Borland and his coadjutors that, upon an examination of the sick returns of the French army for a period of seven years, it was found that at least one-third, or 33 per cent. of its force was annually cut off by endemic diseases. I may add that, when the English landed in Walcheren, there were only eighty-five men alive in a Dutch regiment, which, at its arrival there, three years before, was 800 strong. The annual ratio of mortality of this corps must have been about 31 per cent., or rather more than double the mean ratio of mortality which occurs among troops in Jamaica. Napoleon seems to have known pretty well the real nature of the climate of the Delta of the Scheldt, and of its influence upon strangers; for, in a letter to the Minister of War, in regard to the Walcheren expedition, he says: "We are rejoiced to see that the English themselves are in the morasses of Zealand. Let them be only kept in check, and the bad air and fevers peculiar to the climate will soon destroy their army." The French, it is said, crowed over the expedition with the force of reason, the bitterness of sarcasm, and the playfulness of ridicule.[1]

Some fifty years before, Sir John Pringle, in tracing the influence of paludal exhalations on the British troops in Flanders, during the campaign of 1748, stated that they had scarcely been a month in the cantonment, when the return of the sick amounted to 2,000. One regiment, the Greys, cantoned in Vucht, a village surrounded with meadows, either then under water or but lately drained, were the most sickly. At the end of five weeks they returned about 150; after two months, 260, which was one-half of their number; and, at the end of the campaign, they had in all but thirty men who had never been ill. Another regiment, at Nieuland, where the

29th, the number of sick amounted to little less than 3,000 men. On the 30th, 200 of the artillery, 130 of the 36th, 300 of the 26th, 250 of the 71st, 200 of the 84th, and the whole of the 23d, with the exception of about forty, were in the hospitals. Early in September, there were upwards of 7,000 on the sick list. On the 14th, it was estimated that, of 15,000 in Walcheren, 10,000 were actually sick. Of one regiment alone, the 38th, the sick return was 11 officers and 459 men.—*Davis, A Scientific and Popular View of the Fever of Walcheren*, 9, 12, 14, &c.

[1] Marshall, Statistics of the Walcheren Expedition, Edin. J. xlviii. 313.

meadows had been floated all winter, and were but just drained, returned sometimes above one-half of their number. Another, again, the Scotch Fusiliers, at Dinther, had above three hundred ill at one time.[1] The same distinguished writer informs us that when four battalions of British troops, which had been employed in Zealand during the year 1747, went into winter-quarters, their sick, in proportion to the men fit for duty, were nearly as four to one, which is equal to 800 per 1000.[2]

At Fort Charlotte, in the rear of the town of Nassau, Bahamas, nearly the whole of the 47th Regiment, including women and children, were swept off within a short time. In 1802, 220 out of 300 perished. Of seventy men, sent there in 1818, forty died in six months; besides thirteen women and children out of thirty-seven.[3] In relation to the sickness in Jamaica, Dr. Hunter states that four regiments were sent from England in 1780. They arrived on the first of August. Less than six months after, one-half of them nearly were dead, and a considerable part of the remainder unfit for service.[4]

At the period of the English fleet anchoring in the Rangoon River, on the 10th of May, the troops mustered between 5,000 and 6,000 men. By the end of June, fever had so diminished the number, that, even after reinforcements, scarcely 3,000 troops were left to guard the lines.[5] Of the effects of exposure to the African coast, the results of the Niger expedition will testify. On that occasion three vessels entered the river—the Albert, the Wilberforce, and the Soudam; the first with 62 white men aboard, including officers; the second with 56, and the last with 27. The Albert remained 64 days in the river; the Wilberforce 45 days, and the Soudam 40. In that short space of time, the number of fever cases in the three vessels, with a total force of 145 whites, amounted to 130, or one in 1.12; and that of deaths to 40, or one in 3.6.[6]

At Sierra Leone, an aggregate strength, in eighteen years, of 1,843 white soldiers furnished not less than 2,600 cases, being in the annual ratio of 1.411 per 1,000 of mean strength; while the mortality amounted during that time to 410.2 per 1,000.[7] In the

[1] Dis. of the Army, 56, 59.
[2] Pringle, 65, 66.
[3] Second Report on Quarantine, 59.
[4] P. 11.
[5] Williams, ii. 457.
[6] McWilliams, Med. Acc. of the Expedition of the Niger, 126–128.
[7] Tullock, Rept. of Sickness, Mortality, &c., among Troops on the West Coast of Africa, 8.

Cape Coast command, the aggregate strength, in four years, being 630, the number of cases of remittent fever alone did not fall short of 500, with an annual mortality, from the whole class, of 382.6 per 1,000 of mean strength.[1] In a preceding part of this volume, attention was called to the fact that in the Jamaica command, with an aggregate force, in twenty years (1817–1836), of 51,567 men, the number of cases of malarial fevers amounted to 46,922, being in the proportion of 910 per 1,000 of the mean strength. In the windward and leeward commands, an aggregate strength of 86,661 gave, during the same period, no less than 62,168 cases, or 717 per 1,000. Honduras, where the aggregate strength of the white soldiers, in fifteen years, did not exceed 320 men, there were 221 cases of fever, or 690.6 per 1,000.[2]

A detachment of the 98th Regiment, quartered on a height in Happy Valley (Hong-Kong, China), experienced a mortality, within six months, amounting to 25 per cent. In a locality called West Point, at least one-half of the whole force is supposed to have been lost by death or invaliding. In the summer of 1843, the fever was so fatal, in some parts of Hong-Kong, that it cut off 100 from a total of 300 civilians residing upon the island.[3]

In this city, during the memorable epidemic of 1793, when the population fell short of 50,000, the number of cases may be estimated to have amounted to little less than 11,000, or 200 per 1,000; with a mortality of about 3,500, or 70 per 1,000 of the population.

New Orleans, in 1847, contained a population estimated at 109,000. Of these, 20,000, or 184.31 per 1,000, suffered from the yellow fever. Two thousand eight hundred and eleven cases were reported, and of these 895 died. The population of Woodville (Miss.) amounted, in 1845, to 800. Of this number, 595 passed through the disease in one or other of its various forms; being in the proportion of 74.4 per cent. It is to be borne in mind that 200 of the inhabitants left the village early, so that disease, in fact, bore on a population of 600, and therefore attacked them in the proportion of 99.17 per cent. The population of Cadiz, in 1800, is stated to have been 71,491. Of these, 48,520 were attacked with the reigning epidemic; being in the proportion of 678.7 per 1,000. In

[1] *Ibid.* 20.

[2] *Ib.* Sickness and Mortality of Troops in West Indies, 7, 44, 45, 77.

[3] Barton, Fever of Hong-Kong, Dublin J. No. 24, p. 440, 441, N. S.

1819, the results were scarcely different. In Seville, in 1800, the proportion was larger still; the population being 80,568, the number of cases 76,488, or 949.4 per 1,000, and the deaths 14,685. In Alicant, in 1804, out of a population of 13,000, 9,000, or 692.4 per 1,000 had the disease, and 2,472 died.[1] As I am writing these lines (November 24), the melancholy accounts received from New Orleans, exhibit a mortality there, this year, from yellow fever, of 40 in June, 1,406 in July, 5,189 in August, 1,070 in September, and 139 from the 1st to the 22d of October, making a total, with three in May, of 7,847.

During an epidemic of pernicious or malignant intermittent fever, which occurred in Bordeaux in 1805, and is ably described by Dr. Contanceaux, it is estimated that twelve thousand individuals passed through the disease, and that one-fourth of these died.[2] The fever which swept over the Coromandel in 1809, 1810, and 1811, caused the loss of 106,789 out of a population of 1,828,610. Coimbatore lost in sixteen months 22,451 out of 596,606; Madura, in twelve months, lost 24,626 out of 245,654; Dendigul, in the same space of time, had 21,510 deaths in 29,654 individuals; while at Tinnivelly, the mortality, in the short space of five months, exceeded 38,000 in a population of 690,696.[3]

"It is remarkable," says Lind, "that, in the war which terminated in 1763, the English ships of war which touched at Batavia, suffered more by the disease of that climate, than they did in any other part of India, if we except a malignant scurvy which once raged in the fleet at sea. Soon after the capture of Manilla, the Falmouth, a ship of fifty guns, went to Batavia, where she remained from the latter end of July to the latter end of January, during which time she buried seventy-five of her crew and one hundred soldiers of the 70th Regiment, who were embarked on board of her, not one person in the ship having escaped a fit of sickness, except her commander, Captain Brereton. The Panther, a ship of sixty guns, was there in the years 1762 and 1764, both times, unhappily, during the rainy season. In the year 1762, she buried seventy of her men, and had ninety-two very ill when she left the place. In the year 1764, during a short

[1] See an Essay on the Mortality of Yellow Fever, by the present writer, Charleston Med. J. vii. 463.

[2] Notice sur les Fièvres Pernicieuses qui ont régné à Bordeaux, en 1805.

[3] Med., Geog., and Agricult. Rep. on Fever of Coimbatore, &c., by Ainslie, Smith, and Christy, Lond. 1816, p. 93, &c.

stay, she buried twenty-five of her men. The Medway, which was there in company with her, lost also a great number of her men." The fever was of the remitting kind.[1]

In addition to the facts already adduced, the attention of the reader might be called to the wide diffusion and excessive mortality noted during some epidemics of glandular plague. He might be reminded that, in London, in 1625, it carried off not less than 35,417 individuals, according to Grant,[2] and 46,000, according to the calculation of Short;[3] that, in the same city, the mortality, forty years after (1665), amounted to 97,000,[4] in a population of less than 500,000; that, in 1812, the loss at Constantinople amounted to 159,534;[5] and that in Lyons, in 1628, 29, in a population of some 200,000;[6] fifty thousand persons were destroyed by the same disease. They might also be told that, at Montpellier, the number of deaths in 1629 fell but little short of one-half of the inhabitants who had not left the city;[7] that, in Marseilles, the loss from the memorable pestilence of 1720, amounted to forty thousand;[8] that at Noja, in 1815–16, the number of cases in a population of 5,300 reached 1,474, and the deaths 716;[9] that at Cyprus, in 1760, the disease destroyed 70,000, out of a population of 600,000;[10] again, that at Aleppo, the population of which amounted to some 60,000, it carried off in two years (1761, 62) about 21,800,[11] and that, in 1835, the loss in Cairo was little short of 26,000."[12] These and other instances of like import, might be specially adduced for the object under present consideration; but as the question of the malarial origin of the Oriental plague continues to this day to be a subject of

[1] Hot Climates, 102, 103. Chisholm states that, in the year 1795, the Majestic, 74, Admiral Sir John Laforey's ship, while shut up, during the hurricane months, within a little landlocked bay, situate in the great bay of Fort Royal, Martinico, called *Les trois ilets benits*, and not inappropriately named, by the French, *Gouffre de la Mort*, lost in seven weeks one hundred and eighty-nine men. During the same months of 1796, Admiral Harvey's ship, the Prince of Wales, lay at the same place, and lost ninety-seven men. (Manual of the Climate, &c. of Tropical Countries, 20.)

[2] Natural and Political Observations, &c. made upon the Bills of Mortality, 8.

[3] New Obs. on Bills of Mortality, 274.

[4] Short, *op. cit.* 292; Marshall, Stat. and Mortality of the Metropolis, 66.

[5] Brayer, Neuf. Années à Constantinople, ii. 248.

[6] Papon, De la Peste, Ou les Epoques Mémorables de ce Fléau, i. 184.

[7] *Ib.* 195.

[8] *Ib.* 343.

[9] Morea, Storia della Peste de Noja, 25; Tavola, 3.

[10] Russell on the Plague, 8.

[11] *Ib.*

[12] Boudin (from Gaetani Bey), Geogr. Med. 14.

doubt among those most conversant with the disease, I shall not insist upon them here.

Let this be, however, as it may, the facts already mentioned, no less than the circumstance that, in times of violent epidemics, the early cases, very generally, prove fatal, are with difficulty explained on the supposition of those diseases resulting from the action of any other cause than a morbid poison diffused in the atmosphere of infected localities. Under no circumstances have diseases, undeniably produced by mere changes in the sensible qualities of the air, been found to spread so widely, to occasion so extensive a mortality, and to assume so usually a more malignant and fatal character at the outset of their prevalence.

Autumnal fevers under the influence of various exciting causes.—Individuals exposed to the atmosphere of paludal localities in sickly seasons, or residing in cities visited by malignant and other fevers, become affected with the disease therein prevailing from the operation of a variety of exciting causes. Insolation, exposure to rain, or to a current of cold air, a fit of intemperance, or the free use of stimulating drinks, excessive fatigue, irregularity of diet, a blow, a fall, a surgical operation, the loss of blood, a moral affection, an intercurrent complaint, &c. &c. will bring on an attack of the prevailing fever. It cannot be presumed that all these morbid influences, differing as they do so materially from each other, are capable of producing, unaided by some more efficient and special cause, one and the same disease. Hence, their agency must be limited to the placing suddenly the system, by the disturbance or shock they therein occasion, in a condition required to enable it to be morbidly affected by a cause of a more general character, producing an impression on all exposed to it, and occasioning, when it meets subjects suitably predisposed, a particular and specific set of morbid phenomena similar in all that are attacked. We know of no diseases, except those owing their origin to the action of morbid poisons, that can thus be brought out by perturbating agencies of the kind mentioned; and the fact of the circumstance being observed in regard to intermittent and other fevers of like nature, lends a strong support to the opinion that the cause which gives rise to them consists in a poison of the sort floating in the air of the sickly locality.

The malarial origin of such fevers confirmed by the violent manner the cause often acts.—I might enlarge, in addition, on the circumstance that the efficient cause of fever approximates in nature to all morbid poisons by the suddenly violent and disorganizing effects it produces in the system; for though, when applied in a lesser force and in a gradual manner, it will give rise, as we have seen, to derangements of the viscera, and consequent ill health, without, however, exciting the usual symptoms of marked febrile affections; though, in other instances, the application of the cause occasions fevers of a comparatively mild character, running their course in a few days, yet in some instances death rapidly or even suddenly follows exposure to sources of infection; so rapidly, indeed, that dissection reveals no traces of structural lesion. In all epidemics of yellow fever, while the duration of the disease extends generally to the third, fifth, or seventh days, and in some few instances much beyond, cases occur[1] in which it closes fatally in twenty-four, or even in a very few hours, without leaving any posthumous sign of local determination. Results not very different are observed in that form of malarial disease which has received the name of congestive fever as well as in malignant or pernicious intermittents. The system, in such cases, is at once prostrated, and the patient sinks without having manifested the least sign of a healthful reaction. In no diseases, but those originating from the action of morbid poisons, do we meet with occurrences of the kind.

The cause of fever extends its action to the fœtus in utero.—I might also refer as illustrative of the true toxical character of the efficient cause of fevers—of its analogy to other morbid poisons, and to the fact of its producing, like these, its deleterious impression through the blood, and consequently of its power of solution; that it is known to affect the fœtus in utero, and the infant at the breast. Need I remark, that cases are on record to show that the smallpox has been transmitted from the mother to her unborn offspring;—children coming into the world with well-characterized pustules

[1] Rush, iv. 13; Deveze, 28; Caldwell, 85–87; Lining, ii. 426; Manson, 181; Baxter, Repos. xxi. 3; Gros. 13; Dickson, Chapman, J. iii. 256; Thomas, 89; Berthe, 79; Arejula, 161; Jackson, Fever of Spain, 45; Pym, 60: Palloni, 6; Rochoux, 519, 568; Warren, Fever of Barbadoes, 16; Gilbert, 66, 73; Bally, 272; Caillot, 22, 23; Chisholm, i. 194; Moseley, 440; Imray, Ed. J. liii. 82; Bancroft, 35; Wilson, 7.

over the surface of the body? If the reader doubts it, let him open the works of Jenner, Gregory,[1] Andry,[2] Gardien,[3] Desormeau,[4] Mauriceau,[5] Dimdale,[6] and other writers of easy access, and he will find instances of this transmission stated by high and reliable authorities. He will find that Mauriceau, whose celebrity as an obstetrician is known to all, was himself born bearing the characteristic marks of that disease. He will find, besides, that cases are recorded in which those well-known marks were found on infants whose mothers were not affected.[7] He will even find—but for the authenticity of the fact I will not hold myself responsible—that a Swedish woman having been vaccinated nine days before her accouchement, the child, at its birth, bore on the arms, and on the same spots as the mother, regular vaccine pustules.[8] Extending his inquiries to other complaints, he will find that measles, scarlatina, syphilis,[9] and pellagra[10] are reported to have been thus transmitted. While such is the case with these diseases, we have the testimony of Russell, and others, to the effect that a similar occurrence has sometimes taken place in relation to tertian and other malarial fevers. By more than one author cases are mentioned—and one of the kind fell under the notice of the present writer, in which some of the symptoms of yellow fever were transmitted in this way, from the mother to the unborn babe.

Dr. Ludlow, in his observations on the lake fevers in the Genesee country,[11] relates the following case:—

"Mrs. R. had fever and ague at two different times. During the last stage of her last pregnancy intermittents were very prevalent. Having for several days suffered some of the precursory symptoms, she was, on Sunday afternoon, attacked with a severe paroxysm. Every stage of it was regular and distinct, the paroxysm terminating in diaphoresis. On Monday morning she was delivered of a boy, apparently at the full time. On *Monday afternoon*, at about the

[1] Cyclop. of Pract. Med. iii.

[2] Maladies du Fœtus, Journal des Progress des Sc. Med. N. S. i. 142.

[3] Traité des Accouchemens, 352.

[4] Dict. de Méd. xv.

[5] Observ. sur la Grossesse, &c. ii. 493; Obs. D. C.

[6] Treatise on Smallpox, 279.

[7] Bousquet, Traité de la Vaccine, 167; Fodéré, Med. Légale, v. 397; Jenner, Journal des Progrès, i. 142–6; Deneux, cited by Anglada, Traité de la Contagion, i. 81.

[8] Journal des Progrès, xv. 246.

[9] Journal des Progrès, i. N. S. 142, 170.

[10] Roussel, Traité de la Péllagre.

[11] New York Med. and Phys. J. ii. 94, 95.

same time of day at which the Sunday's paroxysm had occurred, the child was attacked; the cold stage was severe and long; the skin being livid, and the child was thought to be dying. This was followed by the hot stage, and, in due time, by diaphoresis. The paroxysms continued to recur daily for about a fortnight, when small doses of Peruvian bark were given. The disease soon ceased; but, in about a week, the child had two fits more, when the bark again arrested it. The child is now more than two years old, is fat and healthy, and has had no more attacks of the fever. The disease did not recur in the mother after delivery. Both still reside in the same house, which is on aguish ground."

Dr. Ludlow properly remarks, that this case—as others he relates—very satisfactorily proves that the disease is sometimes congenital. It is "remarkable for the regular transfer of the disease from the mother (in whom the susceptibility had been worn out) to the child, who, like others who had never had the disease, was perfectly susceptible of it." The child was affected too soon after birth to justify our believing he did not carry in him the seeds of the disease. Dr. Stokes relates the case of a woman, pregnant and labouring under tertian fever, who felt the child to have convulsive fits on days of pyrexia.

Boudin, to whom reference has so often been made, states that he has several times had occasion to notice the transmission of the disease in question, from nurses to infants at the breast, as manifested by paroxysms of fever, and other *limnhemic* symptoms. "In proof," he adds: "I will add that the infants in question were not under the influence of a primitive intoxication, caught by them in a focus of paludal exhalation, and that my observations were made at the Lazaretto of Marseilles, where malarial fevers, unless imported, are never encountered. Doubtless the most curious fact of transmission I have met with, is the following: The wife of a soldier, recently arrived from Africa, and enjoying good health, undertook to nurse the child of an inhabitant of Toulon. On the third day, the child was attacked with a malarial fever, which was only cured by the sulphate of quinia."[1] In this respect, paludal fevers are on a footing with syphilis, the mercurial disease, &c., all of which are transmissible from a healthy nurse to the infant at the breast. That typhoid fever has been found to be so conveyed, is proved

[1] Fièvre Interm. 193, 194.

by the ulcerations discovered in the intestines of the child.[1] The same has been reported in reference to dysentery.[2]

The cause of autumnal fever appears to be neutralized by the poison of some zymotic diseases which have no effect on common complaints.— Attention might be called also to and much said on the fact observed in Sweden and other parts of Europe,[3] that intermittent fevers disappeared for several years, after the great epidemic of cholera, from their usual habitats; that the observation was made not only in places that had been visited by cholera, but likewise in localities where that disease had never appeared; and that, after the decline of the former, fevers again made their appearance, but in a less malignant form—facts which cannot be readily explained, except on the supposition of the cause of fevers being of the nature contended for, and of its being destroyed or neutralized by the one giving rise to cholera.

It is also to be borne in mind, that mephitic exhalations would appear to have the power of imparting protection to those accustomed to them against malarial diseases, as also against those arising from peculiar meteorations. It was remarked at Paris, that individuals employed or living in the immediate vicinity of the extensive knackery of Monfaucon, did not suffer, to any serious extent, from the cholera, which almost decimated the other districts of that city. In some parts of England, the men whose business it was to attend to the drains were in like manner almost entirely exempt from that disease.[4] Raymond, nearly a century ago, had made similar observations in regard to other zymotic complaints. "I notice," he says, "that workmen employed at inferior (sordides) trades, and in factories filled with vapours, such as starch-makers, tanners, buckskin-makers, &c., are less subject to popular diseases. This observation reminds us of one recorded by Cole de Ballona, that during the fatal plague of the year 1348, individuals working at filthy trades escaped the contagion."[5] The reader cannot have forgotten that during the fatal epidemic of yellow fever which visited the city of Philadelphia, in 1793, it was observed by Dr. Rush,[6] that

[1] Boudin, 196; Rœderer and Wagler, 163.

[2] Zimmerman, Traité de la Dys. 28; J. Frank, Prax. Med.; Watson, 440.

[3] British and Foreign Med.-Chir. Rev. x. 375.

[4] First Report of Commissioners on the State of Large Towns, 160, 164.

[5] Mém. de la Soc. Roy. de Méd. iv. 77.

[6] Works, iii. 83.

the scavengers suffered much less than others differently employed. Similar statements are made by Caillot.[1] Ambrose Pare remarks that during the epidemic fever which prevailed at Paris in 1565, tanners and curriers were in great measure exempt. It may be remarked in addition that at Rome the section appropriated to the Jews, the Ghetto—where the precepts of public hygiene are sadly neglected, and where of course filth abounds—is not entirely but comparatively free from the periodic fevers which afflict severely other and cleaner parts of the city.[2]

It is from a consideration of these facts, and from having observed that in some localities in Mexico and other parts of America, individuals inhabiting filthy districts suffer less from yellow fever than those who live in cleanlier ones, that a physician of our country maintained that what have heretofore been regarded as sources of infection are, on the contrary, conducive to health. Without stopping to inquire whether the exemption alluded to is not the effect of acclimatization or habit on the part of those who are supposed to have reaped advantage from the nastiness in question; and, without arguing the point, whether it would be beneficial or safe to allow such filth to remain undisturbed with a view to counteract the baneful effect of the febrile poison, I may remark that the theory, and the practice founded upon it, are not new, as may be seen in the following passage I transcribe from Lancisi:[3] "Some have entertained a notion that the effluvia of corrupted substances and marshy waters had no manner of noxious operation, because they have read that these very agents were sometimes considered as remedies in some pestilential seasons. Thus, Alexander Benedictus relates that he had heard from a merchant of Candia, that all the dogs were killed during the prevalence of a violent plague, and by order of the physicians thrown about the streets. The air was soon filled with their corrupting exhalations, and their remedial operation immediately restored the place to health. The Sarmatians were accustomed to employ the same means. Very near akin to this story is another, related by George Pictorius, who heard a man from Utopia(!) affirm that, in an epidemic plague, nothing was more wholesome and excellent than, three times a day, to snuff up the

[1] *Op. cit.* 123.

[2] Tournon, Etudes sur Rome, ii.; Carrière, Le Climat de l'Italie, 373; Valentin, Voy. Méd. en Italie, 100.

[3] De Nox Palud. Effl. lib. i. cap. iv. 11, 12.

fumes of a privy or of a sheepfold. So, also, Joseph Quercetanus adduces the case, so familiar to the people of Paris, to wit, that of the nastiness of their streets being considered by many physicians as checking the putrefactive taint of their atmosphere. Nor are there wanting other authorities from very serious writers, collected by Gaspar a Rejes, by which it is shown that bad smells are sometimes valuable auxiliaries of nature."

It is but justice to Lancisi to remark, that he attached no faith to the theory thus set forth, or to the advantages of the practice suggested, and seems disposed to unite in opinion with Rejes, who rejects the practice, and says that the experiments and their authors ought to be banished to those barbarous places where the former were made.

Cause of autumnal fever antagonistic to that of some other diseases.—Nor is this the only instance of antagonism that may be referred to in illustration of the toxicological nature of the cause of malarial fevers. That of phthisis with such fevers, in virtue of which one of these diseases is stated to exclude the other, might, if founded in fact, be usefully adduced; for such an antagonism could not be explained otherwise than on the supposition of the cause of fever consisting of something different from a mere modification in the sensible qualities of the air—of something specifically different from the ordinary causes of disease, and of the tubercular virus in particular, and capable of neutralizing the latter, or of producing a diseased condition of solids and fluids, which exercises a counteracting influence. Originally suggested in Italy, in the early part of the last century, by Lancisi, who found, or thought he had found, that "marshes are salutary to men of certain temperaments, such, for example, as are full of acrid salts, prone to coughing, or slender frames, and predisposed to consumption,"[1] the belief in this pathogenic antagonism was subsequently advocated, in 1783, by Dr. Bang,[2] of Copenhagen, and in 1784, by Dr. Marx, in a treatise on consumption. Taken up anew in England, more than half a century ago, by Dr. Harrison, of Horncastle, Lincolnshire,[3] and ten years after him, by Dr. Wells, of London,[4] it has been revived recently

[1] *Op. cit.* lib. i. cap. v. 10. [2] Selecta Diarii Nosocomii Fredericiana, i. 15.

[3] London Med. and Phys. J. viii. 221, Lond. 1802.

[4] Trans. of a Soc. for the Improvement of Med. and Chirurg. Knowledge, iii. 471, Lond. 1812.

by Dr. Boudin,[1] and by him sustained with considerable ability and much research, but with an evident pretension to originality well calculated to elicit a smile from his English and American readers. But although this antagonism has won the admiration of this able writer, and is well thought of by some of his countrymen, and a few physicians elsewhere;[2] although it be true that phthisical patients are frequently benefited by a residence in malarial localities; that such localities are noted for the rarity or absence of phthisis; that those in which the latter disease prevails are free from periodic fevers; that these diseases have been found to replace each other in the same locality; and that phthisis is becoming common where the country, in consequence of the draining of marshes, is freed from febrile paludal diseases; and although, besides, the power in question is in some measure explained and sustained by the circumstance pointed out by Rokitansky, that an unusual venosity of the blood—a condition observed in malarial fever—proves an obstacle to the formation of tubercles; still, the theory has not yet been satisfactorily established. So far, indeed, from its being true that one of these diseases excludes the other, facts may easily be found to show that, in many regions of country where intermittents and remittents are common, phthisis is as frequently encountered as in non-malarial districts of neighbouring or distant latitudes; while there are many reasons to believe that the absence of phthisis, and its mitigation in some paludal districts, is attributable to the peculiarity of climate, growing out of the thermometrical and hygrometrical conditions of the atmosphere, rather than to any direct and antagonistic agency of the malarial poison there evolved.[3]

[1] De l'Influence des localités Marécageuses sur le fréquence et la marche de la Pht. Pulm. et de la F. typhoide An. d'Hyg. xxxiii. 58, and republished under the title of Etudes de Géologie Méd. sur la Phthisis P., Paris, 1845; *ibid.* Tr. des F. Int. and Remit. 1842, p. 72; *ibid.* Essai de Geogr. Méd. 42, &c.

[2] Tribe, De l'heureux Influence des localités Marécageuses sur la tuberculisation pulmonaire, Montpellier, 1843; Green, New York Journ. of Med. and Surg. 1840; Hennen, Topography of the Mediterranean, 223; Carrière, Du Climat de l'Italie, 336; Heulard, Mém. de l'Acad. de Méd. xiv. 129; Bulletin de l'Acad. de Méd. vii. 213, 305, viii. 931; Haspel, Maladies de l'Algéria, ii. 423; Giannini, Della Natura delle Febbri, i. 115, and i. 237 of translation; King, Southern Journ. of Med. and Surg. Sci. i. 167; Pritchett, African Rem. Fever, 125 (note); Schœnlein, Klinische Vortræge, Berlin, 1842; Wilson, Stat. Reports of the Health of the Navy (British) S. Am. 111.

[3] Lefevre, Bulletin de l'Acad. x. 1041; Southey, Observations on Pulmonary Consumption, Lond. 1814; Forry, Climate of U. S. 265, 266; Sir J. Clark on the Sanative Influence of Climate, 60, 3d ed.; An. d'Hyg. xxxvi. 8, 12; Grant, Sanitary Condition

Dr. Lawson, in an instructive essay on the effects of climate in the production of diseases of the lungs,[1] states, after a thorough examination of the subject, that the mortality of tuberculous affections of the lungs among the English troops, in the various malarious localities they occupy, completely accords with the opinion of Sir James Clark and Dr. Forry; for, although the absolute number of deaths from these diseases may, in the malarious situations, be less than in a more healthy one, still, they will always be more numerous, relatively to those from the purely inflammatory diseases of the lungs, than in the more healthy locality. It is not sure, indeed, that, instead of diminishing the tendency to consumption, malaria will not increase it; for it seems, as we all know, to destroy the balance of the functions, and lessen the tone of the system, and, by defibrinating the blood, deprives the body of the proper nutrition by which the organic functions are sustained—circumstances which all tend to produce or arouse the tubercular diathesis.

More plausible in many respects—better sustained, indeed, by facts, and explainable only by the neutralizing or antidotal effects of different poisons—is the theory of a pathogenic antagonism existing between the cause of malarial diseases, properly so called, and that of typhoid fever, by virtue of which the latter fever seldom if ever prevails in paludal localities where periodic fever abounds, and *vice versâ;* and in the same locality, at different periods, and under particular conditions of soil. Whether the history of these two forms of febrile complaints, in all regions they visit, will bear out the advocates of this antagonism to the full extent of their conclusions, is more than can be now positively averred. Judging, however, from the many facts collected by M. Boudin,[2] who deserves more credit on the score of originality, in relation to this point, than to that of the antagonism of phthisis with malarial fevers, and from others gathered in this country and elsewhere, it is impossible to withhold the expression of the opinion that the theory he has suggested is entitled to a respectful consideration. In the malarial districts of France, Germany, Spain, Algiers, Bone, Senegal, India, and Greece; in the Morea, and the Walcheren, typhoid fever is never or scarcely ever to be found. Nor is it less

of Memphis (Tenn.). Am. J. July, 1853, p. 115; Helfft, Zeitschrift für Gesammte Medicin, B. 8, s. 360, see Edin. J. lxxi. 378; Michel Levy, Bulletin de l'Acad. viii. 939, &c.; Chargellay-Lagarde, Bulletin, xii. 257.

[1] Edinb. Journ. lxii. 57.

[2] See the work cited above.

true, that it is little known in the West Indies. On the other hand, in parts of Europe where typhoid fever is common, intermittents and remittents are rare. At Constantine, in Africa, periodic fevers are scarcely ever seen, while typhoid cases are numerous. In the city of Strasburg, typhoid fevers are common, intermittents rare. In the citadel, the reverse is the case. In Paris, Berlin, Dresden, and St. Petersburg, we find typhoid fever in abundance; but little if any of the other disease. In Denmark, periodic fever drove away typhoid fever, which reappeared when the former had ceased to prevail. In England, much the same thing has occurred; for there, as elsewhere, "it has been noticed that when intermittent fever has yielded to improvements in cultivation, drainage, &c. typhus very commonly succeeds."[1] Individuals arriving from marshy localities, with their systems saturated with the cause of periodic fever, in places where typhoid fever prevails, resist for a long while the cause of the latter, and *vice versâ*. (Boudin, 102.)

In this country, much the same results are obtained. Intermittents once were common in the New England States; they have made way, except in a few spots, for typhoid fever, which is the prevailing febrile complaint of the country. In the south and west the typhoid is gaining ground, and replacing the periodic forms in districts where these once exercised an exclusive sway. Speaking of typhoid fever, Dr. Bartlett says: "I have often met it in Kentucky, where it is sometimes called the red-tongue fever. It is, probably, less common in those portions of the United States which are vitiated by the various forms of intermittent fever, than in those which are exempt from these diseases, although more extensive and accurate observations, than have yet been made, are necessary to settle this point."[2] Another distinguished writer of this country, Dr. Cain, of Charleston, S. C., in a report on the diseases of his State, remarks: "The intermittents and remittents were first observed to lapse into the continued type (typhoid), and finally gave place to the latter altogether. The typhoid now holds, it may be said, undisputed sway over Fairfield, Newbury, Chester, Union, Laurens, Abbeville, and Edgefield Districts, composing a portion of the clay-hill region of the State, but extending somewhat into

[1] Cowan (C.), Report of the Reading Dispensary; Trans. Provincial Association (N. S.), ii. 202.

[2] Bartlett on Fevers, 2d ed. 84.

the middle or sand-hill region, affecting Barnwell, Sumter, Kershaw, and Lancaster Districts."[1]

Alluding to Bedford County (Tenn.), Dr. Lipscombe remarks: "In this part of the county (the northern and western), in former years, intermittents, remittents, and bilious fevers were more prevalent than in the more broken parts of the county. But for the last nine or ten years, these types of fever seem to have been gradually disappearing, and the *enteric* or *typhoid fever* to have taken its place."[2] Another writer, Dr. McNally, reporting on the diseases of Lincoln County, in the same State, says: "Until within the last two or three years, bilious remittent, and intermittent and congestive fevers were the principal diseases which visited this county, especially in the summer and fall. They were epidemic every year, in every part of the county. Pea Ridge was more exempt than any other part; but occasionally they prevailed there as extensively as elsewhere. But for two years past they have been only partially so. During last summer and fall, they *prevailed extensively in those portions of the county not visited by dysentery and typhoid fever.*[3] At Shelbyville, we are told that "the general type of disease, as late as about 1846, was sthenic." "Since 1845 or 6, this characteristic of disease has yielded to the opposite or asthenic form. Since that period, we have had more or less enteric or typhoid fever to contend with; and less and less of the old-fashioned bilious, remittent, and intermittent."[4]

In Alabama, as we are told by Drs. English[5] and Anderson,[6] the same observation has been made. By the former, it is remarked that typhoid fever has pretty nearly superseded the remittent and congestive fevers that once prevailed in that State; and the latter writer, speaking of the diseases of South Alabama, states that autumnal fevers have totally declined there of late years, and that typhoid—a case of which was never heard of till within the last five years—has become a stranger among the inhabitants of this country. Indeed, as observed by Professor Dickson,[7] in "all the

[1] Trans. Am. Med. Assoc. v. 358.
[2] *Ibid.* vi. 321.
[3] *Ibid.*
[4] *Ibid.* 327, 328.
[5] N. O. Med. and Surg. Journ. vi. 168.
[6] Prize Essay on the Summer and Autumnal Fevers of South Alabama. Transl. of Med. Soc. of the State of Alabama, for 1852.
[7] Report on the Blending of the Types of Fever; Trans. of the Am. Med. Association, v. 155; Charleston Med. Journ. vii. 843.

southern medical journals, of recent date, we find it stated that throughout our malarial middle country—and, indeed, though less strikingly, in our lower alluvial districts also—typhoid fevers are becoming more and more frequent in places and settlements, and under circumstances, where, hitherto, the ordinary autumnal remittents and intermittents prevailed exclusively. In certain localities, the congestive forms of intermittents and remittents seemed for years past to be gaining ground, but now appear to be giving way, in their turn, to this newly observed type." It may be proper to remark, that the probability of this conversion or substitution had not escaped the prophetic eye of our great medical philosopher, the late Dr. Drake, who thought it likely that autumnal fevers would decrease, and typhus and typhoid fevers become more prevalent, throughout the whole Valley of the Mississippi.

Doubtless, it is not to be denied that typhoid fever is found occasionally to prevail in malarial districts conjointly with periodic fevers. Ramel[1] described the combination long ago, as having fallen under his observation in some parts of the coast of Barbary and Provence, where intermittents were and are still of common occurrence. It is also found in La Vendée, in Brittany, in sundry valleys near Paris, and other malarial parts of France, where periodic fevers are matters of annual observation. In this country, too, it is on the increase, or already prevails extensively in some aguish or fever localities, as at Memphis, for example, where fevers attributable to malaria have not yet, notwithstanding the appearance of the intruder, lessened in frequency.[2] It may be true, also, that one of the main reasons of typhoid fever being in general less common in rural districts than in cities, as also in the marshes of Corsica, Languedoc, Italy, Algeria, and this country, is to be sought in the sparseness of the population, and in the absence of other morbific agencies which are found in denser communities, and are known to lend a powerful aid to the production and propagation of the disease; while the causes of periodic fevers, which are not found in the latter localities, have not been removed in the former, and continue, therefore, to exercise there their baneful influence.[3] But on the latter point

[1] Memoire sur l'Influence des Marais, &c. sur le Sante de l'homme. Marseilles, an. x.

[2] Grant, Sanitary Condition of Memphis, Am. J. July, 1853, p. 103.

[3] La Pileur, Quelques Objections à la Théorie de l'Antagonisme, An. d'Hygiène, xxxvi. 6, 7.

we have nothing but conjectures, which are met by the fact that typhoid fever exists now paramount, and is becoming a source of terror in many rural districts, where the population is nearly as sparse now as it was previous to its appearance and the decline of its antagonist, and where the other morbid agents above referred to do not exist more abundantly now than they did during the reign of the latter disease. Be this as it may, examples of coexistence are not as frequent as those of reciprocal exclusion; and if they were, would not form a valid objection to the doctrine of antagonism, as we can have no reason to deny the possibility of the occasional existence, at the same time, of the two poisons.

On these various topics much more, I am aware, might be said. I might appeal also to the phenomenon of incubation, and show that the seeds of autumnal fever, like those of other zymotic diseases, occasionally remain concealed in the system during more or less time until brought into activity through the operation of some exciting agent—often, under circumstances which prevent all idea of attributing them solely to the action of the causes to which they are ascribed by the opponents of malaria. Let individuals so situated—who have been exposed to the influence of the febrific cause in some sickly place—remove to a salubrious district, where remittent, intermittent, and yellow fevers are not known, and they will not unfrequently be attacked with one of these diseases weeks or months after their arrival, in consequence of exposure to the action of the sun, to sudden transition from heat to cold, to a shower of rain, or to any other influence by which the surface is chilled after being heated, and perspiration checked; or after a fit of intemperance. Cases of this kind, as we shall see in a future chapter, are not unfrequent. Can we admit that here we are furnished with an illustration of the production of the fever by some sudden change in the sensible qualities of the atmosphere, and independently of the agency of a febrific poison? Had atmospheric vicissitudes, and the other morbific agencies referred to, acted otherwise than as mere exciting causes, we should have had as their product a disease commonly occasioned by them in the locality where the attack has taken place, and not a variety of fever unknown there, but similar to that prevailing at the place whence those individuals came.

Let us notice another occurrence. Two bodies of men arrive in a salubrious locality, the one from a place where typhoid fever prevails, the other where periodic fevers reign. For months, the first

body remain subject to typhoid fever, and are not troubled with intermittents. As regards the other set, intermittents continue to prevail among them, while they are entirely free from typhoid. If so attacked, weeks or months after exposure to sickly localities, by fevers in every way similar to those they have left behind, but unlike the diseases existing in their new place of sojourn, it is evident they must have carried within their system the seeds of such fevers;—those from typhoid districts the seeds of typhoid fever, those from malarial districts the seeds of periodic fever. They are not indebted for the disease by which they are there seized to any morbific agent existing in the place of attack, for nothing of the kind is there to be found. It is the result of exposure elsewhere. Such being the case, the seeds they have thus carried with them, and which are at last called into action, must be of a specific toxical character, for none but morbid poisons can remain long latent in the system; and the atmospheric vicissitudes, through the instrumentality of which the attack may probably have been brought about, cannot have acted otherwise than as a merely exciting agent, seeing, particularly, that other morbific influences, a fit of intemperance, a burst of passion, &c., produce at times the same effect.

I might dwell on the circumstance that as, in the above instances, the cause of the febrile attacks under which the individuals alluded to suffered were evidently of the kind mentioned, and not atmospheric vicissitudes or simple insolation, it would be unphilosophical to attribute the same disease to these latter agencies alone, and ignore the aid of the malarial poison under consideration. I might farther dwell on the process of acclimatization, and draw from what we know on the subject an argument in favour of the doctrine of the toxical nature of the febrile cause. Again, I might point out, in support of the same doctrine, that in some forms of autumnal or malarial fever, individuals who have once passed through the disease, are like those who have suffered from some other zymotic diseases, and, perfectly unlike anything that takes place in complaints arising from heat, moisture, or atmospheric vicissitudes, free from a second attack. But on some of those circumstances, no additional illustration can be required; and, as I shall have occasion to revert somewhat in detail to the others in a future chapter, I must here drop the subject.

As already remarked, enough has been said to show that autumnal fevers depend for their generation on something exhaled

under the influence of certain thermometrical and hygrometrical conditions of the atmosphere, from the soil or the substances accumulated on its surface. Whatever may be our predilections, or even our convictions, relative to the nature of the substance thus produced—and the reader cannot have failed to perceive that, on that score, my opinions are already formed—it matters not, strictly speaking, to the question more particularly under consideration, whether it consists of animalcules, of fungi, of a gaseous or vaporous poison, or whether it must be viewed as a vegetable, or animal, or vegeto-animal product. Nor is it necessary, in order to sustain the position assumed, that I should prove, any more than has been done already, that the poison is the result of the decomposition, fermentation, or putrefaction of organic matter, or of any other known substance; or, that I should establish beyond controversy that it is independent or not of those processes, and the result of some unknown and mysterious agency operating in some equally mysterious manner, or some unascertained materials floating in the atmosphere, but having a predilection for particular localities.

On the animalcular doctrine, as applied to malarial fevers, and other zymotic diseases, we may allow its modern advocates, both those who regard it established beyond the reach of doubt, and those who consider it as most plausible—Bradley,[1] Pleniz,[2] Gattoni,[3] Crawford,[4] Mojon,[5] S. Brown,[6] Holland,[7] Drake,[8] Wood,[9] Grognier,[10] Nott,[11] Grassi,[12] Rasori,[13] and Milroy,[14] to repeat much that was said in former days by Varro, Lucretius, Columella, Vitruvius, Kircher, Valisnicri, Lancisi, Linnæus, Nyander, Ricia, Hartsæker, Moufflet. The same liberty may be extended to the defenders of the doctrine

[1] The Plague of Marseilles considered, 1730.

[2] Op. Med. Phys. 1762. [3] Mém. de la Soc. de Méd. x. 104.

[4] Remarks on Quarantines in Baltimore, Observer, April and August, 1807.

[5] Interno alla natura del Miasmo Choleroso Asiatico, Lucca, 1832. A French translation of this clever essay, by Julia de Fontanelle, was published at Paris in 1832. See Rev. of it in the N. A. Med. and Surg. J.

[6] Dissertation on Bil. Malign. Fever, Boston, 1797, p. 9.

[7] On the Hypothesis of Insect Life, in Med. Notes, chap. xxxiv. Am. Ed.

[8] A Practical Treatise, &c. on Epid. Cholera, 34, 44; Topogr. and Dis. of the Valley of Mississippi, i. 723.

[9] Practice of Medicine, i. 147, 306.

[10] Archive de Statistique du Dept. du Rhône.

[11] New Orleans Med. and Surg. J. iv. 563.

[12] Rapport à l'Acad. Roy de Méd. sur la Peste (Pieces et Doc.) 418.

[13] *Ibid.* [14] Quarantine and the Plague, &c. 11.

which ascribes febrile epidemics, as well, indeed, as cholera and other zymotic diseases, to the toxical effects of various fungi—Pliny,[1] Varro,[2] Ovid,[3] Reinesius,[4] Ramazzini,[5] Lange,[6] Paulet,[7] Hecker,[8] Henle,[9] Cawdell,[10] Mitchell.[11] Either of these doctrines may, if they suit the fancy of their advocates, be adopted. Either, separately considered, or both conjoined, may, for what I know to the contrary, be founded on fact and solid reasoning; though, judging from experiments recently adduced by a distinguished authority, Prof. Leidy,[12] I have strong misgivings on the subject.[13] We may leave the che-

[1] Bk. xviii. chap. xxix.
[2] De Re Rustica, lib. i. cap. 12.
[3] Fast. lib. iv. v. 907.
[4] P. 218.
[5] Const. Epid. Martinensis, anni 1690, i. 3, 4.
[6] Quoted by Dr. J. K. Mitchell, 38.
[7] Rech. Hist. et Phy. sur les Maladies Epid. 443.
[8] On Epid. of the Middle Ages, 206.
[9] Pathologische Untersuchungen, 15; Brit. and For. Med. Rev. ix. 398.
[10] A Disquisition on Pestil. Chol. Lond. 1848.
[11] On the Cryptogamous Orig. of Malarious Dis., Philad. 1849.
[12] Flora and Fauna within Living Animals, Introd. 14, 15.
[13] "Many important diseases have been supposed to originate from parasitic animals and vegetables. The former are not the true entozoa; for these are too large, and may be detected by the naked eye; but they are to be considered to be animalculæ, so small that they cannot be discerned even with the highest powers of the microscope. But, independent of the fact that the existence of such entities is a mere suspicion, none of the known animalculæ are poisonous. At various times, I have purposely swallowed large draughts of water containing myriads of Monas, Vibrio, Euglenia, Volvox, Leucophrys, Paramecium, Vorticella, and without ever having perceived any subsequent effect.

"The production of certain diseases, however, through the agency of entophyta, is no longer a subject of doubt, as in the case of the Muscardine in the silkworm, the Mycoderm of Porrigo favosa in man, &c. But that malarial and epidemic fevers have their origin in cryptogamic vegetables or spores, requires yet a single proof. If such were the case, these minute vegetables and spores, conveyed through the air, and introduced into the body in respiration, could be detected. The minutest of all known living beings is the *vibrio lineola* of Muller, measuring only the 36,000th of an inch; and the smallest known vegetable spore is very much larger than this, whilst particles of inorganic matter can be distinguished the 200,000th of an inch in size.

"I have frequently examined the rains and dews of localities in which intermittents were epidemic upon the Schuylkill and Susquehanna Rivers, but without being able to detect animalculæ, spores, or even any solid particles whatever. I have examined the air itself for such bodies by passing a current through clear water. This was done by means of a bottle with two tubes passing through a cork stopper; one tube dipping into the water, the other reaching not quite to its surface. By sucking upon the latter tube, a current of air passed through the former, and was deprived in its course of any solid particles. Ordinarily, when the atmosphere was still, early in the morning, or in the evening, neither spores nor animalcules could be detected. When

mists to discuss the claims of the various gases to the distinction of producing the morbid effects in question. We may grant, if perchance any one insist upon it, that Sylvius de la Boe had good reason for believing that the cause of fevers is of a saline and sulphurous nature; or that it is acid, as maintained by Ramazzini; or that it consists in an oxide of azote, as believed by Textoris. We may allow Dr. Balme to plead the cause of his septon of *oxygenated azote*, a substance not very different from that which, under the same name of *septon*, enjoyed some celebrity on this side of the Atlantic under the fostering care of our distinguished countryman, the late Dr. S. L. Mitchell, of New York. We may also allow Professor Dunglison to write and teach that vegetable matter has nothing to do in the production of the morbific agent. We may, besides, offer no objections to the opinion of Dr. Warren, and others, who are no less zealous in absolving animal matter from all blame in reference to the effects in question; and we may go so far, for the sake of argument, as to admit, if hard pressed by some fastidious disputant, that the cause is not the production of the decomposition of any kind of organic matter. All I insist upon at present is, that be its nature, the materials from which it is derived, and the process by which it is generated, what they may, the febrile poison is a stranger to, and must not be confounded with, the natural and unchangeable constituent elements of the atmosphere; that it is distinct from, and independent of, mere modifications in the sensible qualities of the latter; that it is suspended and floats in it, and is wafted by the wind; that it possesses an individuality of its own, and serves, by its poisonous properties, to render the air of localities where it is generated or conveyed insalubrious, and a fruitful source of fever.

Other objections urged against the malarial origin of periodic or autumnal fevers.—It can scarcely be necessary to occupy our time

piles of decaying sticks, or dry leaves were stirred up, or the dust was blown about by the wind, a host of most incongruous objects could be obtained from the air: none, however, which could be supposed capable of producing disease. To assert, under the circumstances, that there are spores and animalculæ capable of giving rise to epidemics, but not discernible by any means at our command, is absurd; as it is only saying, in other words, that such spores and animalculæ are liquid and dissolved in the air, or in a condition of chemical solution." (*Leidy, Flora and Fauna within Living Animals, Introduction*, 14, 15.)

in examining in detail the value of various other objections urged against the existence and febrific power of malarial exhalations.

It is not likely that those who believe in the morbific agency of these, and recognize in them the essential or efficient cause of autumnal fevers, will be disquieted by the reasons assigned in opposition by Giannini[1] and others, who remark that the existence of malaria may be disproved on the following grounds: 1. When a morbific matter is introduced into the human body, it manifests its presence by the occurrence of cutaneous eruptions and spots—phenomena which are never observed in malarial or periodic fevers. 2. The morbific matter, when introduced, is usually expelled by means of critical discharges, more particularly by sweats. This evacuation, in intermittent fever, is an effect of the hot stage, and not an expulsive effort to get rid of the miasm. Thus, when this last-mentioned stage is arrested by means of cold immersions, the sweating process does not take place. 3. Morbific matters, inimical to the organism, when introduced into the body, or simply applied to the surface, usually manifest the power of reproduction and multiplication. Through means of this power, a similar disease is reproduced in those who come in contact with the sick. Nothing of the kind takes place in intermittent fever, which produces no morbific matter or excretion, and is not, therefore, contagious. 4. Periodic fevers, which are generally thought to be produced by the paludal miasm, often terminate after the first paroxysm. If the disease arose from the miasm, it is difficult to comprehend how, in so short a time, that cause could be expelled, considering that all morbific matters that produce fever in the living system, remain in it some days before being expelled. Peruvian bark and cold affusions cannot be supposed to act in these cases by neutralizing the alleged poison, inasmuch as the same means produce the same curative effects in periodic fevers arising from other and totally different causes. 5. Diseases produced by foreign matters introduced into the system are all communicable by contact. So far from being the vehicle of contagious diseases, the atmosphere decomposes all contagions. Reasoning from analogy, therefore, we may conclude that the atmosphere would do the same as regards the alleged miasm, did it exist. 6. If there existed a paludal miasm, its effect would be specific and uni-

[1] Vol. i. 230, 231.

form. So far from this, the effect of the pretended miasm is multiform. It gives rise, not only to periodic fevers, but also to cachexia, marasmus, abdominal obstructions, dyspepsia, chlorosis, dropsy, &c. 7. The cause often operates too suddenly to allow us to believe that it consists in a miasm. If such were the case, we should be led to admit that in a few hours the miasm has been introduced, that it has circulated in the lymphatic vessels, and next in the arteries and veins; or that, after having penetrated the lungs, in opposition to the known fact that these organs receive no other gases than oxygen, it has been able to exercise an action on the nerves—a result which does not obtain in diseases produced by morbific agents of a poisonous character. Besides, the same effect may be produced by any other cause, since it is proved that an attack of intermittent fever is often brought on by simply sleeping one night in the open air in places free from marshes. Now, as in instances of this kind, we cannot refer the disease to any other influence than that of cold on the nervous system, we need not attribute the other phenomena to a different cause.

To these objections it may be remarked: 1. That all morbific poisons, introduced into the system, do not manifest their presence therein by cutaneous eruptions and spots; as proved by that producing hooping-cough. 2. Diseases caused by such poisons terminate often without the occurrence of critical discharges—that by sweat particularly—while, on the other hand, autumnal fevers, of the remittent or continued kind, which belong to the same class with intermittents, are frequently *judged* by such discharges. 3. Nothing has as yet been said calculated to prove the contagious character of cholera; the origin of which, from a morbific agent introduced into the system, admits of no doubt. Many poisonous animal matters, when introduced under the skin, cause dangerous and even deadly diseases of a specific kind, without imparting to the system the power of reproducing a similar matter endowed with poisonous qualities. 4. The cure of an intermittent fever by cinchona, after the first paroxysm, is no proof of the non-existence of a malarial poison; as there is no reason for disbelieving the possibility of a disease being produced by a small portion of miasmal poison, terminating with or without a critical discharge, in a short space of time. Some poisons require for their elimination a certain and fixed number of days; but it does not follow that in all the duration must always be prolonged. Besides, in some fevers, due to the

same causes, or to influences closely allied to them, the disease runs a long and definite course; and, when established, is not arrested by the means mentioned. If arrested by cinchona, the effect is due, as is also the cure of intermittent fever, to the control exercised by this remedy, and some other therapeutic agents, over all affections that assume the periodic type, whether those affections be produced by one set of causes or another. Giannini himself, admits that typhus, petechial, pestilential, and other forms of fever, which he regards as the effect of morbific poisons, may be, and often are cut short, or greatly abridged in their duration, by cold immersions. 5. It is not correct to say that the atmosphere is never the vehicle of contagions; since exposure to smallpox or like complaints, without contact, is generally followed, in unprotected individuals, by an attack of the disease. The poison, in these cases, is certainly conveyed from the sick to the well through the medium of the atmosphere; and if the latter does not always decompose it, immediately on its escape from the diseased body, there is no reason to deny that it may fail to do so in regard to the malaria producing periodic fevers. 6. The diseases produced by malaria are specific and uniform—each variety of the poison producing a different form of fever. The affections enumerated in proof of the multiform effect of marsh poison are only sequelæ of the specific disease, or the result of the gradual and long-continued action of the cause. 7. And lastly, even if it were true that no other gas than oxygen found entrance into the lungs—which is far from being the case—the occurrence of fever after slight and momentary exposure to marsh air, is no proof of the non-existence of miasm; for the small-pox and other diseases depending on morbific matters introduced into the system, have often been occasioned by a few moments' exposure to individuals labouring under them.

To only one more objection shall I devote a few remarks. It has been said, as a proof of defective and erroneous reasoning on the part of the advocates of the mode of origin of autumnal fevers under consideration, and as an evidence of a culpable violation of the rules of induction, that such individuals insist that the same cause—*not known even to exist at all*—may and does produce different and distinct diseases in different individuals exposed under the same circumstances. Now, to this I have only to say, that I cannot at this moment recall to mind a single writer of sane intellect, or whose opinion is worth notice, who has ventured and pertinaciously insisted on such an

assertion. That some have believed, and continue to believe, that the same cause—miasmata—produces what other pathologists regard as different and distinct forms of fever, is perfectly well known. But such writers, themselves, while believing in the identity of the cause, believe also in the identity of all the fevers in question; holding these to differ from each other in nothing but the degree of their violence, the organs that may be implicated, and other circumstances of like import. Hence the remark cannot apply to them. By others, nothing of the sort, so far as I can find, has been advanced. By them, it has doubtless been said, very correctly, as I think, that miasmata or exhalations produce different forms of fever; but while doing so, they have not wished to be understood as maintaining that those exhalations are always identically the same in their nature. So far from this, they think themselves justified in the belief that, considering the great diversity of effects produced, as evinced by the phenomena and anatomical characters noticed in those several forms of fever, poisons exhaled from various sources of infection differ materially in composition and nature. The exhalations from our wharves, from the timber of ships, and some other sources, produce yellow fever, and never intermittent fever. Those from swamps, marshes, &c., give rise to periodic fevers of different grades—never to yellow fever properly so called. In Paris, and other parts of France and Europe, and in various sections of our country, some effluvia appear to give rise to typhoid fever. In several localities of Ireland, Scotland, and England, certain exhalations have occasioned a peculiar form of fever, which, from one of its noted characteristics, has been denominated the relapsing fever. The morbus Hungaricus, of some centuries past, bore but a feeble resemblance to the Athenian plague, described by Thucydides; to the Oriental plague of the present day; to the sweating sickness, and to the black death of the 14th century; or, again, to the typhoid fever of Paris, the typhus of London, or the typhoid remittent of our negro alleys; while none of them resemble exactly the true yellow fever. Whatever view we may take of the contagious character of typhus, it is not to be denied that it arises often from miasmatic exhalations of some sort.

Even among fevers that are strictly speaking of malarial origin, some difference occurs in their symptomatological and other characters. The jungle fever of India is not exactly the same in form as the remittent of Africa; the fever of Batavia differs in some

respects from the bilious remittent of this country, or of the Gambia. The febrile diseases of Rome are not precisely similar to those of the West Indies or Batavia; and the Walcheren fever differs in like manner from both of these, or from that of Breskau. In France, the fevers of Rochefort do not present the very same characters as those of Bresse or of the plain of Forez.

The Mediterranean remittent, described by Sir William Burnett, is somewhat unlike the periodic fevers of England and of northern Europe. It differs in some particulars from that of Greece, an account of which is found in the justly celebrated books of Epidemics of Hippocrates, or of Algeria, or the Morea. Our negro fever of 1822, and the Bunker Street fever of New York, were too unlike yellow fever on the one hand, and common periodic fever on the other, to be supposed to have arisen from the agency of precisely the same poison as these.

This probable diversity in the nature and composition of the miasms in question, as exhibited by the diversity of the febrile phenomena they produce, has called the attention, and met with the assent of many writers of former and present times. It is referred to by Lancisi;[1] approved of by Rochoux,[2] Ferrus,[3] Desland,[4] Littré,[5] and many others of equal note; and particularly insisted upon by Twining,[6] J. M. Smith,[7] and Monfalcon.[8]

Those fevers, though bearing a close family resemblance to each other, are all more or less different in their phenomena, mode of progression, and anatomical characters, but yet are all avowedly the offspring of miasmal effluvia. Here, the cause giving rise to each form, though included with others under the generic name of miasma or malaria, is also regarded as different in its composition or in the proportion of its component parts. This is what is affirmed; and not the absurdity sometimes charged on the advocates of the malarial origin of fevers, that a cause identically the same may and does produce different and distinct diseases in individuals exposed to it under the same circumstances. Individuals exposed

[1] *Op. cit.* cap. xi. 34–36.

[2] Recherches sur les Différentes Maladies, &c. 135.

[3] Dict. de Médecine, 1st ed. viii. 68.

[4] Dict. de Méd. Pratique, vii. 73.

[5] Œuvres d'Hippocrates, ii. 578.

[6] Diseases of Bengal, ii. 288.

[7] On Epidemics, 48, 67, &c.

[8] *Op. cit.* 65, 69. See also Rev. of Devèze, Med. Repos. xxi. 187; Forry, *op. cit.* 285, 291; Desportes, Bulletin de l'Acad. v. 380.

to the same cause, under the same circumstances, will have the same disease. If the diseases produced differ, the cause must be different too; because the same morbific agents cannot occasion different effects, any more than different agents can occasion the same effect.[1]

If the causes of those diseases were precisely the same, and the latter consequently of similar nature, they would be convertible into each other, and show themselves in the same place; yellow, or typhoid, or relapsing fever would, under particular circumstances, be converted into a remittent or intermittent fever, and *vice versâ;* and, where the one form of febrile complaints exists, the other forms would appear too. But, notwithstanding all that may be said to the contrary, experience shows that such is not the case; a true yellow fever has never been converted into a typhoid or an intermittent fever. They may, and do often mix or combine together; their types may blend, but they remain independent of each other, as will be dwelt upon on some other occasion. Where intermittent fever prevails, yellow fever frequently never shows itself; and in places most visited by yellow fever, the other is never or very seldom seen.

In the Islands of Dominica, Demerara, and St. Lucia, common miasmal fevers prevail annually, while the yellow fever appears at irregular intervals. In Barbadoes, on the contrary, the latter disease is of frequent occurrence, and the former scarcely known. Bilious remittent fever, in its worst forms, is as prevalent in India, in Eastern Europe, and in all parts of Africa, as in our Southern States, the West Indies, and the western coast of Africa; yet, not-

[1] Cases are not wanting to show that the same locality will, under particular hygienic and atmospheric conditions, produce different forms of fever. Witness the following: At a short distance from Drieuze, in France, is situated a large pond called Lindre Basse, which, for special purposes, is made to undergo certain changes. It is two years full and one year empty. The first year it is only half-filled—the second year quite so; and the third, after the fish have been taken out, it is dried and cultivated. During the first year, intermittents prevail; during the second, typhoid fevers take the place of these, while during the third, malignant carbuncular diseases succeed to both.[1] This order of succession has been regularly observed during the last twenty years.[2]

[1] Ancelon, Mémoires sur les Fièvres Typhoides périodiquement développées par les émanations de l'Etang de Lindre Basse. Nancy, 1847; Méller, Mém. de l'Académie de Méd. xiii. 657.

[2] A change in the rotation in 1848–49, has altered the order of succession of these diseases.—*Comptes Rendus*, 1850.

withstanding, the yellow fever is seldom if ever seen there; while it is very common in the others. Bilious remittents and intermittents prevail as extensively, and with as much violence, in the interior of unhealthy countries as near the sea; the yellow fever, on the contrary, is confined within narrow limits, and is generally observed on the sea-coast, and along navigable streams, and never extends far into the interior. The yellow fever, as we have seen, has often been known to arise from the foul exhalations of ships; no instance, so far as I have been able to ascertain, has as yet occurred of epidemics of remittent or intermittent fevers having broken out at sea, or been traced to the decayed timber, or dirty bilge-water, or fermenting cargo of a ship. If they break out there, they have invariably been brought from elsewhere. The yellow fever is, strictly speaking, a disease of cities, or of places containing a dense population. That it has sometimes attacked small towns, villages, plantations, or rural districts, is true; but such instances are rare, and have always occurred along large watercourses. The bilious remittents or intermittents, though occasionally seen in city localities, prevail more generally in the suburbs or adjacent country, to which the other never extends. Although it often happens that an epidemic of yellow fever is accompanied with a like prevalence of remittents and intermittents in the surrounding country, or, perchance, in the infected place itself, the occurrence is not constant; for instances are found when, during severe visitations, the country or suburbs have remained free from common malarial fevers. On the other hand, in some very unhealthy seasons, both in the West Indies and in our Southern and Middle States, when the fevers have prevailed most extensively, the yellow fever has not made its appearance.

To those who reject these views as unphilosophical and absurd, it may be asked whether they are more so than others to which they are forced to give their assent, respecting phenomena witnessed every day. Take the process of putrefaction, as an example. All animal substances give by this process the same products, but they do not follow exactly the same laws, and present the same phenomena. These are often dependent on a difference in the proportion and nature of their constituent principles. Fourcroy, a high authority on this subject, remarks: "Each substance has its particular mode of comporting itself while undergoing the putrefy-

ing process." Each gives out a particular odour, and each produces a somewhat different effect by its action on living beings.[1]

But we ask for *proofs*, not *assertions*, and unsupported denials; and say, to all who condemn a doctrine which we regard as resting on the most solid foundations, and which, as every one must acknowledge, has received the sanction of the highest professional authorities, and enumerates among its advocates the larger number of medical inquirers, that, to succeed in their efforts, something more will be required at their hands, than the mere assertion that those who advocate that doctrine labour under a most egregious and unpardonable error, and are neither more nor less than open to the charge of hazarding a sheer hypothesis at complete variance with the results of daily observation and the commonest principles of a sound philosophy, and worthy only of those unenlightened minds who have lagged behind during the onward march of scientific knowledge. As for myself, whether it be from obtuseness of intellect, ignorance of important facts, or other circumstances of like import, matters not; but I cannot help thinking that, notwithstanding all that may be affirmed to the contrary, the doctrine of malaria is, to say the least, entitled to our most respectful consideration; that enough has been adduced to satisfy the minds of ordinary inquirers, that those who admit the claims of that doctrine to be so viewed, are not far from the truth; that nothing, at least, has so far been done or said to prove the contrary; and that the opponents of the malarial origin of autumnal fevers would confer an inestimable service on the profession, if, instead of contenting themselves, as they have but too generally done, with summarily disposing of the subject with a few words of contemptuous denial, or dwelling on morbific influences, not one of which can, unaided by a more efficient cause, produce any form of that disease, they were at once to refute all that has been said in support of that origin; to show, clearly and positively, how and in what manner their opponents have erred and continue to err; and to furnish us, in detail, with facts and arguments as numerous and strong as those they seek to overthrow.

In the meanwhile, at the risk of incurring the charge of repetition, I must, before concluding the subject of malaria, lay down a few propositions, which, it appears to me, we are warranted in

[1] Julia de Fontenelle, *op. cit.* 116.

drawing from a survey of all the facts and statements which have been passed in review.

1. The doctrine of malaria, though of ancient origin, and very generally admitted, has encountered, and continues to encounter opposition.

2. The appearance of autumnal or periodic fevers, where there are no marshes, properly speaking, does not disprove the existence or agency of malaria in the production of that class of disease, inasmuch as there is nothing to hinder morbid exhalations from being furnished by terrestrial surfaces of a different character, and no writer of any reputation has denied the fact of such occurrences.

3. The constant association of these fevers with peculiar characters and conditions of localities, and their absence or cessation where these characters and conditions do not exist, or, having existed, have ceased to do so, through the operation of artificial or other means, lead to the opinion of the evolvement from those localities of some peculiar morbific poisonous substance from the soil, or the materials by which it is covered; and of the connection, as cause and effect, between this exhaled substance and the diseases in question.

4. The inability of the chemist to detect this malarial poison in the atmosphere of sickly localities, and to point out its nature, does not disprove its existence, inasmuch as other substances, the presence of which cannot be doubted, equally escape detection.

5. Atmospheric heat alone will not serve to account for the production of periodic fevers, of various grades and types; for these fail to appear in seasons when, and in localities where, the thermometer ranges higher than at other seasons when, and in places where, they prevail extensively. Besides, instances are not rare when fevers have stopped, though the heat continued unchanged, and apparently from the influence of a very high range of temperature.

6. Nor can terrestrial or visible atmospheric humidity account alone for the effect under consideration. Though fevers often break out or prevail during wet weather, they usually cease when this humidity is at its height, and reign most generally during the drying process—often during very dry spells of weather.

7. Fevers of the kind mentioned cannot be due simply to a high dew-point, for they exist and are rife when the latter is not higher than in healthy seasons.

8. For reasons assigned, the efficient cause of fever cannot be sought in any peculiar modification in the electrical state of the atmosphere. This fluid, by its excess or deficiency, may and does no doubt exercise an agency in the matter; but that agency is evidently limited to placing the system in a proper condition to receive the impress of a morbific cause; to furthering, when deficient, the formation of the latter, or, when in excess, to neutralizing or destroying its effects. But, in all cases, the presence of such a cause is required, before autumnal fevers can be produced.

9. The same may be said of atmospheric vicissitudes. These may and do often occur, without periodic fevers being the consequence; and, conversely, those fevers frequently occur without appreciable vicissitudes.

10. The attack comes on too suddenly, and, not unfrequently, after too transient an exposure to evident sources of infection, without the possible concurrence of any other influencing agency; at other times, it occurs too long after a residence in, or visit to places where the disease is known to prevail, to be the effect of any other cause than a morbid poison introduced into the system.

11. The opinion of the existence and agency of such a poison will appear the more natural, when we find that the danger of an attack is generally proportionate to the proximity to localities where these diseases prevail;—the other agencies being the same beyond as within the sphere of their prevalence;—that vessels on a sickly coast remain healthy so long as they do not approach the land; that they become again healthy by removing to a short distance, or by merely shifting their position; and that individuals from on board, who land, and those especially who sleep on shore, are almost certain of being attacked.

12. The development and prevalence of fever on board of ships, when other vessels in the vicinity remain healthy; its occurrence only in limited parts of the under decks; and its being arrested by a proper system of expurgation, lead to the opinion of the generation and existence there of a morbific poison.

13. The innocuousness of some marshes, of ships in a foul state, or of surfaces bearing a strong resemblance to others in which periodic fevers prevail, is no proof that exhalations issuing from sickly places or ships, are not the cause of the disease; for, besides that the latter is often traced to some source of decomposition in a way to leave no doubt as to the agency of these, the exemption

may be justly attributed to the existence, in some instances, or to the absence, in others, of a variety of necessary and concomitant circumstances. Fevers do not prevail beyond a certain altitudinal range; they require a certain amount of heat, and that this heat should be continued during a certain length of time; they require, besides, a certain amount of terrestrial humidity, no more nor less; or, at least, that the soil should have been well saturated before being exposed to the prolonged operation of the solar heat; also, a close and still atmosphere, and other contingencies adverted to. Without these they do not appear to show themselves, how favourable soever to their generation may be the condition and nature of the soil or the substances by which it is covered or permeated.

14. The malarial doctrine receives farther support from the well-attested fact, that the cause of fever is carried by the wind from paludal and sickly localities to places situate at considerable distances; the latter places remaining healthy so long as they are to windward of the others, and becoming sickly when they are to leeward. In such cases, the wind cannot have carried an amount of heat or humidity sufficiently different from that existing before to account for the effect produced. Nor can it act by occasioning a much greater amount than usual of atmospherical vicissitudes. Hence the cause, to be thus wafted from one place to another, and to give rise to a particular disease, similar to that of the locality from whence it proceeds, must consist of a material poison, mixed with, or suspended in the atmosphere.

15. Equally favourable to the correctness of the belief is the effect resulting from the upturning of soil in hot weather. The injury resulting from this operation, as illustrated by a wide diffusion of malarial fevers, and the great mortality often occurring on the levelling of streets, digging ditches and canals, cutting down bluffs, caving in of river banks, &c., cannot be accounted for on the principles advocated by the opponents of the malarial doctrine, and are more easily explained on the supposition of a poison exhaled from the decomposed organic matter contained in freshly exposed earth.

16. The effect of partial draining; of exposing a virgin soil to the action of the sun; of the imperfect desiccation of the soil after an overflow, and other kindred occurrences, in producing or increasing fever; and the converse effect of complete draining, of covering a marshy surface with water or sand; or masses of decaying

organic materials, or any other unhealthy place, in the same way, or with earth, in putting a stop to fever, lead to the same conclusions.

17. Agreeably to no other doctrine than the malarial, can we explain the greater sickliness of marshes formed by the mixture of salt and fresh water; for this mixture cannot increase any of those influences to which others attribute diseases thus produced or aggravated; while we can understand that the mixture may act injuriously, by furthering the decomposition of the organic matter which it saturates, as well as by the reciprocal destructive action of fresh water on the living beings contained in sea-water, and reversely, the destruction by the latter of those contained in fresh water.

18. The limitation of the disease to a restricted locality, to a few streets or buildings, to one house, or side of a house, or one room, &c., bespeaks, as do like occurrences on shipboard, the existence of a local cause of infection, and consequently the exhalation from this of a morbific agent.

19. The opinion of fever being due to the introduction into the system of a morbid poison, floating in the atmosphere of sickly places, receives a strong support from the analogy existing between their symptoms and anatomical lesions, and those resulting from the introduction of putrid organic matter into the circulation, or the inspiration of putrid vapour.

20. The arresting or mitigation of fevers in sickly localities by a resort to proper hygienic means, disinfectants, washing, excessive heat, &c., lead to the same opinion; for these means operate in the same manner on other morbific causes the effluvial nature of which is undoubted, while they have no efficiency in cases of diseases arising from other agencies.

21. The effects of trees, walls, hills, buildings, curtains, &c., in arresting the transit of the cause of fevers, are equally favourable to the idea of that cause being a malarial poisonous substance contained in the atmosphere.

22. Still more conclusive is the destruction of that cause by frost, for the latter cannot produce its beneficial effects by an action on any of the other agencies to which fevers are ascribed; while it is known to exercise the same destructive influence over other causes, of a character similar to the one from which fever originates.

23. The wide prevalence of autumnal fevers in certain localities;

the great mortality to which they sometimes give rise; and the diffusion of some forms of them over a very large expanse of country, at a time, too, when the sensible qualities of the atmosphere do not appear to have varied in any important point from what they are in healthy seasons, afford an additional reason for attributing them to a toxical agent floating in the atmosphere.

24. The transmission of the disease to the fœtus in utero; its production from the internal use of the waters of marshes; the inability of such waters to sustain life in fish and other animals of the kind; the undermining effect of a malarial atmosphere on the system; the production by it of a state of cachexia, and its influence in shortening the duration of life, indicate the existence and agency of a poisonous substance transmissible, in the one case, like other morbid poisons, by the mother to the child she bears in her womb; in another case capable of solution in the water which helps to its generation; and in others, again, endowed, like well-known poisons, with the power of gradually occasioning peculiar and injurious changes in the blood and vital organs.

25. The neutralizing influence exercised by the poisons of some zymotic diseases over the agent producing malarial fevers, would seem to indicate the existence of a close analogy between the latter and the former. The same remarks are applicable to the pathogenic antagonism existing between malarial and typhoid fevers.

26. Lastly, from all that we can gather respecting the origin, mode of progression, and phenomena of autumnal fevers, the nature of the localities they visit, the circumstances under which they appear, the agencies which promote their development, or retard or arrest their progress, we may conclude, without fear of error, that everything tends to connect the morbific agent, of which autumnal fever is the offspring, with the products of the decomposition of organic materials; requiring as it does, for its generation, the action of the very same agencies which are necessary for that decomposition. Like the latter, it requires the presence of the above materials; like the ordinary decomposition of these, the febrile cause requires for its generation a more or less prolonged continuance, and a certain degree of atmospheric heat; it requires, also, a certain amount of moisture. In the one as well as the other process, an excess of this moisture prevents or arrests its progress; in the one as well as the other, a total absence of the same produces a like preventive or destructive effect. The generation or diffusion

of the febrile cause is promoted by a calm and close state of the atmosphere, and retarded, prevented, or modified, by free ventilation, elevated situations, and a pure quality of the atmosphere. Like ordinary decomposition, the process by which the febrile cause is produced, is retarded by cold, and arrested by frost, as well as by intense heat.

Now, as has been said before, when we find the cause of fever requiring for its development the action of the very agencies which are necessary to insure the development of the gaseous products of decomposition; when we find that without these agencies, applied in certain proportions, neither those gaseous products nor the efficient cause of fever will manifest themselves; that in all instances in which the latter is produced, as shown by the occurrence of fever, materials capable, when acted upon by the agencies in question, of giving rise to the evolvement of the gaseous products of decomposition—organic matter in various conditions and states of modification—exists; that the total absence of those materials, whatsoever be the degree of heat, and of terrestrial and atmospheric moisture, carries along with it an equal absence of fever; and that, when once formed, the gaseous products of decomposition, as well as the febrific cause, are cut short by frost or intense heat, and the process by which they were generated arrested; when, I repeat, all these circumstances are borne in mind, we can have no reason to doubt the propriety of admitting that the febrile cause presents a close analogy to the above-mentioned gaseous products; and that if, in regard to the latter, heat, humidity, and other agencies acting in given proportions and in concert on dead organic matter, give rise to certain products which assume the gaseous form; and if the process of the generation of these, and their existence in the atmosphere is destroyed by frost or intense heat; the febrile poison which requires for its generation the action of the same agencies, as also the existence of a kindred matter, and is besides arrested in its formation, and destroyed when formed in the way mentioned, must necessarily consist also of some modification of a similar kind of gaseous substance; in other words, that autumnal fevers must be the offspring of a malarial morbid poison, and not the effect of modifications in the sensible qualities of the atmosphere.

CHAPTER V.

PNEUMONIA AND AUTUMNAL FEVER COMPARED IN REFERENCE TO THEIR CAUSES, MODE OF PROGRESSION, SYMPTOMS, ANATOMICAL CHARACTERS, AND THE CIRCUMSTANCES BY WHICH THEY ARE INFLUENCED.

The causes of autumnal fevers and pneumonia are different.—In the preceding chapters, I have endeavoured to prove, by facts and arguments, that autumnal fevers, in their several varieties and forms, from the simple intermittent to the malignant yellow fever, are produced by specific toxical agents, exhaled under the modifying action of certain contingent influences, heat, humidity, &c. from organic substances during the process of decomposition; and those agents I have, in imitation of Dr. Irvine, who first borrowed the term from the Italians, designated by the generic name of malaria. Whether the attempt to establish, in a satisfactory manner, the correctness of this generally admitted but somewhat contested belief has been successful or not, must be left to the decision of the reader. Convinced, however, from all I have seen or read, that the majority of those who have investigated the subject with due accuracy, learning, and impartiality, will affirm the conclusions to which I have arrived; unable to discern, in the writings of the opponents of the malarial origin of fevers, anything calculated to weaken, in the least, the force of what has been adduced in its support; and fully satisfied that none of the other agencies to which those diseases have been, at times, ascribed, can give rise to their phenomena, I have no hesitation in regarding that mode of origin as a fact placed beyond the possibility of doubt.

Now, if while entertaining these sentiments—while holding autumnal fevers to be the morbid products of the poisons in question—we lend a willing ear to the speculations of those, who, reviving a long-exploded hypothesis, contend that the same dele-

terious agents which produce fevers will also produce pneumonic inflammation; and if, from this supposed similarity of origin, we argue that pneumonia is really and substantially nothing more than a peculiar form of remittent and intermittent fever, we shall be inevitably led to the conclusion, that malarial exhalations may and do give rise, in some localities, and under certain circumstances, to inflammation of the lungs. Nor is this all. Experience, as I have endeavoured to demonstrate in a former chapter, teaches that pneumonia occurs, and even prevails extensively, under circumstances when malaria cannot, in the most distant manner, be suspected to have been an efficient and necessary agent in its production—as, for example, in places where the poison is not evolved, or, if so, is harmless; or in seasons of the year when, supposing it to have existed or produced its characteristic effects before it has been destroyed through the influence of frost or other disinfecting means. In all such localities and seasons (as the reader cannot fail to perceive), it follows that the disease, when it occurs, must be due to the operation of morbific agencies, different from those to which autumnal fevers have been distinctly traced. The conclusion is inevitable, and, if while taking it into consideration, we admit the reality of the peculiar effects assigned to malarial exhalations—if we consent to regard these as prolific causes not only of autumnal fevers but of pneumonic inflammation also—we shall be compelled to recognize the possibility of the latter disease being, not only in different localities and in different seasons of the year, but everywhere, and at all times, the offspring, indiscriminately, of two sets of causes, which, on examination, will be found to possess very dissimilar characteristics and properties. Hence, we shall have the rather anomalous phenomenon of one and the same disease sometimes springing from the operation of a toxical agent—the more ordinary, or indeed legitimate effect of which is the production of autumnal fevers—and at other times resulting from morbific influences, which, whatever be the variety of the diseases they really produce—a question we have nothing to do with at present—have never, so far as can be discovered, occasioned, alone and unaided by the former agent, an attack of pure remittent, intermittent, yellow, or kindred fevers. To conform ourselves to the language of our opponents, we shall have an example of one form of periodic fever, *i. e.* pneumonia, produced by a particular cause—malaria—which gives rise also to other forms assumed by that

Protean disease—remittent, intermittent, and yellow fevers; while in other and more frequent instances, the aforementioned form, pneumonia, springs from the action of other and very different morbid influences, prolific in inflammations of that sort, but differing widely from malaria in this, that they cannot give rise to the fevers in question. In a word, admitting the views referred to to be well founded, the so-called pneumonic form of autumnal fever may be produced by either of two sets of causes; the one of which, at the same time that it occasions that effect, is the active agent in the production of the more legitimate form of the disease; while the latter form of disease—true autumnal fever—results from the operation of but one of these sets of causes—malarial poison—and has never yet been satisfactorily traced to the baneful influence of those other agents, which are acknowledged to produce the pneumonic form.

Now, it is not presuming too much to say that there is not one solitary case, authentically recorded, and adduced by an individual competent to decide in matters of the kind, in which genuine pneumonia can be truly said to have been produced solely by the well-ascertained cause of autumnal fever, and which can thereby enable any one to demonstrate, in a satisfactory manner, the connection, as cause and effect, between malaria and that disease. That a congested state of the lining membrane of the bronchial tubes, allied to inflammation, exists in many cases during the cold fit of an ague, may be and is doubtless true. The strong, sonorous râle, which totally disappears on the accession of perspiration, indicates this condition, and has been noticed too often to be denied. There may exist, also, during the cold stage, some congestion of the lungs, which occasionally may approximate to the first stage of pneumonia. But these conditions are the effects of the mere concentration of blood in the internal organs, and occur in all complaints attended with a chill. They do not amount to positive inflammation; and, besides, do not present themselves in other forms of malarial fevers in which the chill is feebly marked and not repeated. None, indeed, of the circumstances connected with the evolvement and dissemination of malaria, whose legitimate effect is the production of autumnal and periodic fevers of various grades and types, in localities where the proper materials of decomposition exist, will be found to prove instrumental in causing pneumonia or other diseases of a kindred character. Nor can it be proper to look to

the products of such decomposition to account for the occurrence of pulmonary inflammation in malarious localities and seasons, unless we feel disposed, at the same time, to ignore, in such places and at such periods, the morbific influence in the production of the disease, of the causes which are acknowledged to produce that same disease in situations where, and at times when, malaria does not and cannot be admitted to float in the atmosphere. In other words, before doing so, it will be necessary to prove that in localities or seasons noted for the evolvement of malaria, the ordinary causes of pneumonia do not prevail, or, if they do, remain perfectly harmless, and allow their rival in mischief to produce alone effects, to which, under other circumstances, they themselves give rise. It is scarcely necessary to remark, that there is nothing in the atmosphere of a malarious locality capable of excluding the ordinary causes of pneumonia; and in maintaining that they there assert their sway as freely as when the air is not tainted by morbid effluvia, we do not sin against the canons of that system of philosophy on which some of our opponents lay so much stress. Now, if such morbid influences exist as well in malarious as in other localities; if nothing can be pointed out capable of preventing the morbid effects they produce in the latter from being produced also in the former, it is certainly needless to have recourse to such effluvia for the purpose of accounting for the occurrence of that disease; for when the latter occurs under such circumstances, it may fairly be attributed, not to malaria, but to the operation of its ordinary causes. The conclusion will appear still more natural when we bear in mind a fact already adverted to, that none of the conditions of locality, which are inimical to the formation of malaria, as well as none of the hygienic means by which the evolvement of the poison is arrested or diminished, have the effect of preventing or lessening the prevalence of pneumonia. The disease continues to present itself as if no changes had been made in the surrounding circumstances, and we are justified in presuming that the same causes which give rise to it *after* the extinction of malaria have produced it *before* that extinction.

But let us admit, for the sake of argument, that in all I have advanced on this subject, I err. Let us admit that sporadic pulmonary inflammation—catarrhal or parenchymatous—is produced occasionally, or often, by the malarial poison, and that the epidemic forms of the disease are due to the same cause; I am not sure that

the admission will afford much help to those who feel disposed to argue, from this circumstance, that pneumonia is only a peculiar form of periodic fever. In the first place, the conclusion, if well founded, would only apply to those particular cases which occur where malaria is known to be evolved; for, as we have seen, and as is acknowledged, inflammation of the lungs presents itself under circumstances when it must be due to the operation of other morbific influences, and cannot, therefore, be viewed as closely connected with the fevers in question. In the second place, whether pneumonia be produced by one or the other of the causes, from the action of which it is well-ascertained to originate, it is, to all intents and purposes, the same disease; it is simply pneumonia, and nothing else. If produced by malaria, the same thing would occur —the morbid effects produced on the lungs would be the same, as if they were the result of any of their ordinary causes. The disease would not be, because thus produced, autumnal fever, but simply pneumonia. If unmixed with unusual morbid elements, it would be governed by the same laws as other cases; it would be recognized by the same symptoms; would present the same anatomical characters, and would call for the same treatment; while, in cases presenting the unusual elements above alluded to, these would constitute an addition to the original elements of the disease, by which the latter would be complicated and more or less modified, without, however, being altered in its essential characters.

2. We have seen, in a preceding chapter, that the prevalence of malarial fevers, of various grades and types, is restricted within certain localities; that, in many instances, the area of these infected places is very limited in extent; that while the disease prevails in one spot, individuals who reside at a short distance, and abstain from visiting that spot, escape; that by removing from one part of the same city to another not far distant, or from one end or side of a house to another, or from a lower to a higher story, the disease may be avoided; that ships, by shifting their position from one part of a sickly port or shore to another close by, are often found to lose the fever from which, before the change, they had suffered severely. We have seen, on the other hand, that in certain localities, the crews of vessels that had been exempt from fever so long as they remained at only a short distance from land, were attacked, sometimes to a man, as soon as they ventured ashore; and that individuals who had enjoyed good health while avoiding infected

city or country localities, were attacked, with an almost unerring certainty, in consequence of visiting or passing through them. Now the physician who should undertake to collect facts to prove that the sphere of prevalence of pneumonia is as narrowly circumscribed as it is sometimes found to be in fever, would have an ungrateful task to perform. We nowhere hear of this disease attacking a large number of the residents of a limited spot, of a part of a street or house, and leaving every one in the close vicinity of that spot, in the next street, or in the adjoining houses, perfectly unscathed. Nor need we fear to predict that the medical writer who ventures on the assertion that pneumonia has frequently been observed to attack the occupants of the lower rooms of houses, or the basement wards of a hospital, and to scrupulously respect those who dwell above; that all the inmates of one end of an asylum, hospital, prison, or house, have been struck down by inflammation of the lungs, while those occupying the other parts of the same building have remained untouched—and that, too, not during one season, but during a succession of seasons—will run great risk of giving but an unfavourable opinion of the authenticity of his facts, or the soundness of his judgment. We do not hear of the crews of ships ridding themselves of the disease in question, by shifting their position, and anchoring at the distance of a few dozen yards; or of vessels, which before had been healthy, becoming, by a reverse change or the removal of an intervening ship or other object, suddenly visited with the disease. Neither do we find a large number of instances on record of vessels which were free so long as they remained under sail, or at anchor at a short distance from an infected shore, being filled with pneumonic cases the moment they approached close to the land, or sent their boats to explore the mouths of rivers, &c.; or, again, of scores of individuals, who had remained healthy while residing at a short distance, being attacked with pleurisy or pneumonia in consequence of jumping over the barricades, and promenading about the streets of an infected spot.

3. It has been seen that the cause of autumnal fever is wafted by the wind; that the disease prevails in localities situate in such a way as to receive, through that means, the malarial exhalations evolved from local sources of infection; while other localities placed in a contrary direction, though at a very short distance, escape more or less completely. It has been seen, also, that the effect does not depend upon the particular point of the compass, abstractly

considered, whence the wind happens to blow, but upon the position the suffering locality occupies relatively to marshes or other sources of morbid effluvia. Again, we have seen that the disease, thus produced, will be arrested in, or diverted from, its course by a hill, a row of trees, a wall, a curtain, or other such causes of obstruction; and, on the contrary, that places heretofore healthy have become infested with fever, by simply removing such obstructions and allowing a free access to some particular wind. The history of pneumonia furnishes us with nothing of the kind. It presents no example of the disease being produced or prevented in this way; of its causes being wafted by a particular wind passing over an infected locality; of its course being impeded by hills, trees, walls, and curtains; or of cases of it being multiplied by the removal of these. That cold winds will, as we shall see, produce, under particular circumstances, cases of pneumonia, is doubtless true; but it should be borne in mind that it matters not whether such winds, in order that they may occasion that effect, pass over sickly or healthy localities, while their real agency in the matter is without difficulty accounted for by the sudden production of one or more of the ordinary causes of the disease.

4. We have seen that the overflow of the sea, of lakes, of rivers, or other sheets or streams of water; the establishment of mill-dams; the digging of canals; the levelling of streets; the partial draining of marshy surfaces; the clearing of new land, and the like, are fruitful sources of fevers. We have seen that these are produced by exhalations from masses of decaying or green timber, or of other vegetable as well as of animal substances, separate or combined, in a state of decomposition. Where, may it not be asked, shall we find well-authenticated instances of pneumonia produced by such occurrences, unless they bring along with them some of the ordinary and well-known causes of the disease? Apart from these causes those occurrences are harmless, so far as thoracic inflammations are concerned.

5. Facts in large numbers have been adduced to show the salutary effects of complete drainage in putting a stop to, or in diminishing, the prevalence of fevers. While such is the constant result of this measure in regard to those diseases, no change is effected so far as concerns pneumonia, which generally continues to show itself as if nothing had occurred. We have seen that the same salutary effects, as regards fevers, have followed the complete flooding, by

artificial or natural means, or the washing of insalubrious localities. So far, nothing has been pointed out calculated to show that from such occurrences have resulted like salutary effects in respect to pneumonia, the number of cases of which does not seem to be smaller when the country at large is deluged with water, than when it is but partially covered; or after a heretofore sickly locality has been rid, through the effect of rain or otherwise, of all sources of malarial exhalations.

6. We have seen that autumnal fevers, the offspring of malarial exhalations, prevail sometimes to an extraordinary extent in certain localities, and that, on particular occasions, they spread over a wide expanse of country, striking down at once, or in rapid succession, a large portion of the population, and causing among them a considerable mortality. We have seen, also, that while in certain localities such fevers prevail extensively, and occasion a great loss of life, in other places, situate close by, and differing little from the former in climate, altitudinal range, and other particulars, they attack but few, and give rise to a small proportional mortality; that in some of the West India Islands, for example, the deaths among the troops have amounted to 104.1 per 1,000 (Tobago), while in others (St. Vincent's and Barbadoes) they have not reached 12 per 1,000. Nothing of the kind will be found to hold good as regards pneumonia, which in ordinary times is, comparatively at least, a disease of unfrequent occurrence; prevails in the same place to very much the same extent at each return of its appropriate season; or, if it varies in that respect, does so on account of corresponding changes in the sensible qualities of the atmosphere; prevails to an equal extent in each of the several places where fevers are so unequally distributed; occasions everywhere much the same rate of mortality; and, at periods of its widest epidemic diffusion, neither attacks by far so large a number of individuals, nor causes so immense a loss of life, as we find to be the case with regard to the other disease.

7. It is a fact placed beyond doubt, that malarial fevers, whatever be the form they assume, are more likely to be the result of exposure, in infected localities, to night air, by which the poison may very naturally be understood to be condensed and rendered more prolific of disease. During the various epidemics of yellow fever in this city, the baneful influence of night air was manifested in such innumerable instances as to leave no doubts on the subject in the minds of physicians conversant with the disease, by some of

whom it has been recorded in emphatic terms.[1] Dr. Merrill, already cited, in a private communication to the present writer says, in reference to the yellow fever of Natchez: "I know of no instance in which the disease was ever taken from exposure in the daytime, and then returning home to the country." This only confirms an observation very generally made at every epidemic return of this form of fever in tropical and temperate climates. The same holds good in regard to remittents and intermittents everywhere, abroad or at home; even under circumstances when the greatest attention is paid to guard against the action of ordinary morbific influences.[2] I am not aware that pneumonia, in regard to the peculiarities attending its production, is governed by any law of the kind. Its causes are operative by day as they are by night. When proper precaution is taken, during the latter period, to shield the body from their effects, the disease will be easily avoided. But when the same precautions are not attended to during the day, the disease is as apt to follow then as at any other period. Even admitting this not to be true, experience teaches that night exposure is not more dangerous, so far as regards this disease, in a malarial than in a non-malarial locality; the condition of soil, temperature, and other contingencies being the same in all; whereas, the contrary is the case in reference to autumnal fevers, which follow on such exposure only in certain localities.

8. Nor are we to lose sight of the fact pointed out by very reliable authorities, and which experience everywhere confirms, that the production of fevers is favoured by the state of sleep. Dr. Rush, in his account of the epidemic of 1793, says: "A great proportion of all who were affected by this fever, were attacked in the night. Sleep induced direct debility (which he afterwards called debility by abstraction), and thereby disposed the contagion (malaria), which floated in the blood, to act with such force upon the system as to destroy its equilibrium, and thus produce a fever. The influence of sleep, as a predisposing and exciting cause, was

[1] Rush, Fever of 1793, p. 50; Works, iv. 18, 85; Deveze, 113, 114; J. K. Mitchell, 53, 54; Chapman, Phil. Med. and Phys. J. viii. 319; Emlen, N. A. J. v. 327; Caldwell on Miasm, 507; do. on Malaria, 128, 144; Wood, i. 314.

[2] Grant, 58; Moultrie, 62; Valentin, 228; Lempriere, ii. 113; Dariste, 218; Rollo. 144; Arnold, 60; Celle, 292; Caillot, 260; Bally, 590; Bryson, 217; Brocchi, 260; J. Johnson on Trop. Cl. 73; Rigaud de L'Isle, in do. 312; Gilbert, 81; Barton, Fev. of Hong-Kong, Dublin J. (N. S.) xii. 344–347.

often assisted by the want of bedclothes suited to the midnight or morning coolness of the air." That such an agency has been called in question by Dr. Deveze (114, 115), and others, is doubtless true. But by reference to some of the older and many later and contemporary writers, from Lancisi to the present day, we shall find statements of facts observed in Europe, Asia, Africa, and America, which confirm the views of our eminent countryman.[1] Speaking of the remittent and intermittent fevers of Peru, Ulloa long ago remarked: "*Quand ces fièvres regnent dans les Quebradas* (or deep valleys) *il suffit d'y séjourner pour en être pris; qu on y dorme de nuit ou de jour, on ne les évite pas*."[2] Nor can the effect be matter of astonishment. Every one knows that certain functions are modified in a more or less marked degree by the state of sleep. Dr. Edwards has shown,[3] and the same observation was made before by Sanctorius and Keill, that the state in question has a tendency to increase the perspirable process. At the same time, the nervous energy is diminished, the circulation is less rapid, calorification is lessened, other functions are performed with less energy, and the whole system is in a state of relaxation. In a word, the body is in a condition calculated to impair greatly the elimination of the malarial poison, and thereby favour its morbific impression on the system. That the state of sleep may, for the same reason, render the body more susceptible to the action of other morbid agencies—even of those which give rise to pneumonia—and that hence the attack, in some instances, may have been due to exposure to cold during that state, no one is justified in denying. But such cases are comparatively seldom encountered; and in fenny districts or infected localities, persons who fall asleep in the open air at night, during the sickly season, are not more than ordinarily liable to attacks of pneumonia, or at any rate not more so than if no cause of infection existed; while in other localities, during corresponding seasons, but where such causes do not exist, sleeping in the night air may not

[1] Lancisi, *op. cit.* 77; Bancroft, 86, 87, 100, 172; Lind on Seamen, 75; *Ib.* on Hot Cl. 192, 233; Wood. i. 144: Simons, 18; Bryson, 218; Wallace, Edinb. J. xlvi. 273; Monfalcon, 196; Alibert, 235; Folchi, N. A. J. vii. 258; Clark's Notes on Italy. 10; Blane on Seamen, 230; Edinb. Rev. xxxvi. 546; Celle, prat. des Pays Chauds, 298; Watson, Pract. 450; Brocchi, Stato Fisico de Roma.

[2] Ulloa, Mémoires Philosophiques Hist. Physiques concernent la Découverte de l'Amerique, i. 245.

[3] On Physical Agents, 102, Am. ed.

be indulged in with impunity unless the same precautions are used as would be necessary in the daytime.

9. We have seen that the development of fever may be prevented, and its progress arrested, by the judicious use of disinfectants—by smoke, the sprinkling of lime, the lighting of fires; by the clearing and washing of gutters, sinks, and sewers; by the removal or covering over of sources of infection. No such means have ever, to my knowledge, prevented the development or arrested the progress of pneumonic inflammation, among the inhabitants of any locality, large or small. To this, let me add that in malarial localities, where fevers are rife, and exposure early in the morning is almost sure to be followed by an attack, it has been found useful to avoid going out on an empty stomach or without taking a small quantity of ardent spirits, or a cup of hot coffee. From this has doubtless arisen the custom, so prevalent in some parts of tropical countries—in Martinique, Guadaloupe, Guiana, Cayenne, &c.—of taking a cup of strong infusion of the latter on rising in the morning. By means of this substance, the bitter principle of which bears some analogy to that of cinchona, intermittent fevers, as we learn from Moreau de Jonnes, are frequently prevented. The same effect is obtained by bark itself, or some of its salts taken in the same way. When the French army encamped in the morasses of Mantua, the soldiers were all made to take, at the dawn of day, a portion of tincture of bark, and from this precautionary measure, which was long ago recommended by Lind and other writers, the most beneficial results were obtained.[1] These precautions will serve very little purpose against pneumonia.

10. We have seen that fevers are in very many instances the result of a solitary and transient exposure to an infected locality; indeed, that the large majority of those who thus expose themselves, run an imminent risk of an attack. In regard to pneumonia, not one probably in thousands would be in danger from such exposure in any locality; and in those so attacked, after a visit to an infected spot, the disease would easily be traced to a disregard of precautionary measures absolutely necessary everywhere to guard against the disease, and which have no connection whatsoever with

[1] Blane, Dissertations, i. 221; Bryson, 227; Aubert Roche, Mem. sur l'Acclimatement, An. d'Hyg. xxxiii. 23; Monfalcon, *op. cit.* 232; Celle, Hygiène des Pays Chauds, 347, 367; Moreau de Jonnes, Hygiène Militaire des Antilles, 67; Lind on Hot Climates, 317, 318; Thevenot, 95; Caillot, 264; Rollo, 147.

the special causes of autumnal fevers, while the danger would not be greater in sickly localities than elsewhere.

11. It has been stated, on respectable authority, that in both the West and East Indies, places where the mangrove and mancinellas grow luxuriantly, the most unhealthy spots are those in which the roots of those plants are only occasionally and partially under water; and that in tropical climates the existence of a large quantity of astringent plants, the bark of which is rich in animal matter combined with tannin, is generally connected with the development of fever. What reason have we for believing that these circumstances exercise an influence on the development of pneumonia?

12. Epidemics of fever are on very many occasions attended with a wide-spread disturbance in the health of the population at large—a considerable number of individuals, though not labouring under the fully-formed disease, exhibiting one or more of its symptoms in a mitigated form, and bearing the marks of the cause having produced an impress on the system, as shown by the condition of the eyes, the secretions, the alimentary canal, the brain and nerves, the blood, &c. We do not find such a result to obtain during seasons when pneumonia is most prevalent.

13. Neither do we find that in those seasons the lower orders of animals or plants suffer in a way corresponding to that they are often found to suffer in times of fever epidemics.

In fine, the more we examine the question at present before us, the more inclined we must be to adopt the opinion, that pneumonia is never produced simply by the introduction into the system of a malarial poison by which the atmosphere of an infected locality is polluted; for, when the disease attacks individuals exposed to the influence of such places, it may be traced to the operation of other agencies, which prevail contemporaneously with the poison in question, and cannot fail to exercise their baneful effects in an impure, as they do in a pure locality. Nor can we greatly err, when we affirm that if those agents produce pneumonia in places where the malarial influence does not extend, where malaria is never evolved, or where its evolution, if it has taken place, has been completely arrested, as proved by the constant absence or cessation of its legitimate effects—fevers—it is to them we must look for the true cause of the development of the disease when it shows itself in infected localities, and not to the exhalations by which the atmosphere of those localities may be contaminated. So long as care is

taken to avoid the action of those agencies, exposure to malaria may produce fevers; but no quantity of it received into the system will occasion pneumonia, which, in fever localities as elsewhere, owes its origin to the operation of the same causes.

Causes of Pneumonia.—As regards the nature of the morbific influences in question, it will not be necessary to say much in this place. Every one conversant with the subject, whether in or out of the profession, knows full well, that pneumonia and all other kindred affections of the thoracic organs, are very usually due, not in one place and in one season only, but in all places and at all times, to atmospheric vicissitudes, to exposure to cold, especially when the body is hot and perspiring, to currents of cold air, and the like.[1] Dr. Barton, of New Orleans, who has paid much attention to meteorological subjects, informs me, in a private communication, that "Pneumonia is most apt to occur at variable temperatures from 20 to 50—with a low dew-point, and great drying power, producing rapid evaporation from the surface of the lungs and skin." Add to these sundry physical and local causes—violent exercises of the voice, irritating vapours, and asphyxiating gases, blows on the chest, wounds of the lungs; the translation by metastasis of gout, rheumatism, cutaneous diseases; the transfer of inflammation or purulent deposits after extensive wounds in various parts of the body, or important surgical operations.[2] Experience teaches all this; and teaches, besides, that the prevalence of pneumonic inflammation in each system of climate increases and decreases in proportion, not as the evolution of malaria increases or decreases, but as the seasons are contrasted; thus maintaining, as Dr. Forry has correctly pointed out, an unvarying relation with the extreme range of the thermometer as connected with the seasons. In other words, we find that the ratio of those diseases is highest in proportion as the difference between the temperature of summer

[1] Hildebrand, Inst. Pract. Med. iii. 194; Bouillaud, Dict. de Méd. Pract. xiii. 359; *Ib.* Méd. Clin. ii. 151; Grisolle, 147; Swett, 80, 81; Williams, Cycl. of Pract. Med. iii. 407; Chomel, Dict. de Méd. xxv. 164; Andral, Méd. Clin. i. 512; Barthez et Rilliet, i. 115; Pinel & Brichetau, Dict. des So. Méd. xliii. 396; Laennec, i. 546; Forbes's Transl. 225; Daniel Monro, ii. 247; W. Phillip, 204; Wood's Pract. ii. 42.

[2] See Sir C. Bell's Surg. Operations; Guthrie on Gunshot Wounds; Forbes's Laennec; Williams, Cyclopædia; Grisolle, 142; Rouppe, 44; J. Frank, 816; Lieutaud, i.; Portal, Anat. Med. v. 72; Andral, Clinical Med. i. 512; Erichsen, Med.-Ch. Trans. xxvi. 29.

and winter increases, and lowest in proportion as it grows less; that the stronger the impression on the system by the high temperature of the former season, the more susceptible the lungs are rendered, so far as regards inflammatory complaints, to the morbific agency of the opposite seasons; that those diseases are invariably less prevalent in the moist and changeable climate peculiar to the sea-coast and large lakes of this country, where malaria is abundantly evolved, than in the dry atmosphere of the opposite locality, where the febrile poison is not largely produced. Hence, if pneumonia and pleurisy are more frequently encountered in the Middle and Southwest regions of the United States than in the cold and variable climates of our Northern and Eastern States, the explanation is not to be sought in the faet, that the cause of periodical or autumnal fevers exist in many sections of those regions, but in the circumstance that individuals are placed under the control of the law above alluded to; *i. e.* the greater impressibility of the thoracic organs to eold occasioned by the previous influence exercised by long-eontinued summer heat; and that the effect is equally well marked, whether the loeality where these diseases are found, be or be not the seat of febrile affections, or characterized by the kind of soil prone to the production of the latter; and are likewise noticed in climates where, from the peculiarities of the soil, the nature and proportionate duration of the seasons, and the range of the thermometer and hygrometer, they never have originated, and in all probability never can originate.

Another writer, who has investigated the subject on even a larger scale than Dr. Forry—Dr. Lawson, of the English army, and whose conclusions are drawn from the results obtained among the British troops in the different stations scattered over the greater part of the earth, remarks, that the frequency of inflammatory diseases of the lungs, and the amount of the resulting mortality, are proportional to the prevalence and activity of the (not malarial poison, but) refrigerating causes relatively to the powers of resistance of the constitution.[1]

Not very different is it in relation to catarrhal complaints, which, as we have seen, Dr. Copland and others have elassed among the produets of malaria. Except, perhaps, when it presents itself in the garb of what has been denominated influenza—and then it is appa-

[1] Edinb. Med. and Surg J. lxii. 50.

rently due to some wide-spread and travelling meteorological change, of the essence of which we know nothing, but certainly bearing no analogy to miasmal exhalation—catarrh is evidently the offspring of those atmospheric influences to which other thoracic irritations are due; while the idea of its dependence on the specific cause of periodic fevers will be found destitute of solid foundation. On this subject let the following remarks of Dr. Forry suffice:—

"That catarrhal fever has not the remotest connection with malarial causes, is demonstrated by the statistics of our troops. As the ratio of intermittent and remittent fever is about five times higher in our southern than northern latitudes, and as that of catarrhal affections is twice as high in the latter, it follows that, as the results are in an inverse proportion, no relation of cause and effect is discoverable. As the classification of Dr. Copland, however, limits the production of catarrhal fever to 'temperate ranges of atmospheric heat, aided by moisture,' it may be said that it applies only to our northern regions. In diametrical opposition, then, to this view, it is found that in the winter, when no 'miasm from decayed vegetable matter arises, the ratio is twice as high as in summer, and that the annual ratio in the moist climate of the lakes, and the coast of New England, is not more than half as high as that of the dry climate of the region remote from large bodies of water.'"[1]

All this, let it be said *en passant*, differs somewhat from the results obtained in regard to autumnal fevers, which, in temperate regions, where the temperature of the several seasons are generally strongly contrasted, often prevail most when a very long hot summer succeeds to a very cold winter; and which, on the other hand, spread more extensively in tropical regions and in the southern regions of this country and Europe, where the seasons are not contrasted, or are so to a limited extent, and which require for their development and dissemination meteorological conditions, for the most part the reverse of those mentioned in regard to pneumonia.

But be this as it may, the experience of the world shows that although it is not always possible, in all individual cases of pneumonia, and under all circumstances, to arrive at a precise knowledge of the cause of the disease, owing to the difficulty of obtaining definite and correct information from those attacked, or from the indis-

[1] Climate of the U. S. 305.

tinct manner in which the morbid agent has impressed the system; yet, in the larger number of instances, the disease is due to well-ascertained injurious influences, not one of which can produce, or has ever produced, intermittent, remittent, bilious, or yellow fevers, or bears the least analogy to the morbid poisons which give rise to these maladies. And it is not a very great stretch of assumption to suggest, that in those instances in which the cause cannot be satisfactorily ascertained, it would, if discovered, be found to be allied to the former influences, and to have nothing in common with the latter. True it is that writers have speculated on the possibility of inflammatory affections of the lungs being, under particular circumstances, produced by the introduction of certain morbid poisons into the circulation, through the agency of respiration or otherwise; and it may be argued that, if we admit this to be the case in some instances, the advocates of the malarial origin of pneumonia will think themselves justified in maintaining, that what is true of one or more poisons, may be supposed to hold equally good in regard to the cause of autumnal fevers. Such, for example, has been said to be the effect of the poison of the rattlesnake, which, it is affirmed, gives rise to pneumonia; and we know that epidemic catarrh or influenza, in which the disease extends sometimes to the substance of the lungs, as well as epidemic pneumonia, are supposed by respectable authorities—among whom, in reference to the latter complaint, we find no less a name than that of Laennec[1]—to be due to a peculiar miasm floating in the atmosphere; the reason assigned being the universality of the prevalence of those affections, and the difficulty of tracing them to any of their ordinary causes. In all this, however, we can discover nothing more than conjecture. In the first place, the statement respecting the agency of the poison of the rattlesnake in producing pneumonia, to say the least, needs confirmation. By those most experienced in the matter, it is stated that when a rabbit or small animal is poisoned by this snake, it gives no evidence of feeling pain, and generally for some minutes appears to be well—his ears then begin to droop, giddiness and uneasiness follow, and the animal falls; the pupil becomes dilated, slight convulsions ensue, and death closes the scene in about fifteen minutes from the accident. On examination, it is found that the red colour of the blood in the part affected has been destroyed.

[1] Traité de l'Auscultation, i. 547.

The muscular fibres and cellular substance for two inches around the puncture are black, and so is the blood in the veins leading from the wound; that in the heart is darker than natural, and does not coagulate so firmly as when the animal has been killed by a blow on the head; the supply of blood to the brain is deficient; but neither this organ nor any other solid parts show signs of injury, except near the wound, which swells sometimes very considerably.[1] In man, the symptoms are longer in developing themselves, and the disease runs a more protracted course; but the phenomena are similar in kind, and in no instance evince anything indicating the existence of pneumonic inflammation. If pulmonary symptoms occur, and the lungs are found to present, after death, marks of morbid change, the effects are due, in this and other kindred cases, to simple congestion, a condition very generally encountered in affections characterized by, or combined with a defibrinated state of the blood.[2]

Again; of the morbific agent which gives rise to influenza we know little. Indeed, if we knew more, it is not certain that our information would lead to anything of much importance relative to the present inquiry; inasmuch as that agent, whatsoever be its nature, affects primarily the mucous tissue of the lungs; and pneumonia, when it occurs, is only consecutive, and not the direct effect of the cause producing the primary disease. Moreover, in regard to epidemic pneumonia, nothing as yet adduced proves its malarial origin. If sporadic cases of pulmonic inflammation can and do arise from the operation of causes totally unconnected with the existence and evolution of miasmal exhalations, there is no reason why the epidemic form of the disease should not be assigned to the same or similar agencies acting more extensively, in consequence of a universal state of predisposition brought about by a peculiar, insensible, and unfathomable condition of the atmosphere. The diffusion of the disease over large tracts of country; its prevalence at diversified seasons of the year, under diversified conditions of the sensible qualities of the atmosphere; its visiting with equal force localities of different or even opposite characters and conditions; its appearing, even at sea, far beyond the reach of any possible source of exhalation; are inimical to the theory of its miasmal origin; while the argument

[1] Stevens on the Blood, 138.

[2] See a communication on "The Bites of Venomous Serpents," by Dr. E. Hallowell, in Trans. of Philad. Col. of Phys. i. N. S. 394.

derived from the circumstance of the impossibility of discovering an evident cause to account for epidemic pneumonia is met by the fact already referred to, that sporadic cases, which it would be next to an absurdity to attribute to malaria, spring up without its being possible to trace them to any evident cause. At any rate, if the morbid agent giving rise to this and the preceding form of disease must really be viewed as nothing more than a species of malaria, that malaria can bear no analogy to the one producing autumnal fever. More likely is it that the cause of diffusion of the disease is meteorological, and assimilable to the terrestrial fluids of electricity and magnetism, and that its operation is limited to predisposing the system to be acted upon by other agencies.

Dr. Merrill, to whose Essay attention has been especially called, disclaims, as we have seen, the disposition to deny that pneumonia sometimes appears as an idiopathic affection—a form of the disease which he seems never to have encountered in his practice, and only admits, because, to use his own language, such is the result of observations made by others, who are certainly not less qualified to form a correct judgment in the premises. Even while upholding his favourite theory respecting the supposed connection between that disease and periodic fever, he regards the former, wheresoever it may appear, and whatsoever form it may assume, as being excited into action, or, in other words, as requiring for its development, the agency of the sudden transitions of temperature which occur on the approach of winter. Entertaining such views on these subjects; recognizing as a well-established fact the existence of an idiopathic form of the disease, distinct from that which he supposes prevails most generally—sometimes sporadically and frequently as an epidemic—he will not refuse to acknowledge what, indeed, as we have seen, experience establishes beyond the possibility of doubt, that in northern latitudes and in elevated situations, where malarial fevers do not occur at all, or, if they do, show themselves only during a limited portion of the year, and are cut short by the intervention of frost; where, therefore, the cause producing them is never evolved, or, being so, is destroyed on the approach of winter; and where, as a consequence, pneumonia cannot be regarded as the offspring of that cause;—this disease, which prevails in such localities at all seasons of the year, and is more particularly rife under circumstances perfectly incompatible with the development of febrile diseases, must necessarily be referred to the atmospheric influences above

enumerated. We must presume, also, that he will not refuse to regard the idiopathic form of pneumonia, when it occurs in truly miasmatic regions, whether in the south, the north, the east, or the west, as the offspring of a cause differing from that giving rise to periodic fevers, and with these to that form of the disease which he views as closely connected or identical with the latter; which cause cannot be other than the atmospheric influences which, as we have seen, produce inflammation of the lungs elsewhere, and, according to his own saying, is necessary to excite into being the symptomatic form. The inference is inevitable; for, were the cause not acknowledged to be of a different character, there would be no ground upon which to establish the distinction between an idiopathic and a symptomatic form of pneumonia.

Now, if the disease is thus viewed as arising from the agency of a distinct cause—often in these and in northern latitudes, and sometimes in the south and malarial districts generally; if, at the same time, it is acknowledged (if not in actual words, at least by implication) that, were it not for this cause—*i. e.* the sudden transitions of temperature which occur at the approach of winter—the thoracic viscera would not, even in the symptomatic form of the disease, be called upon to bear the burden of local disease, and would escape risk of inflammation; and if, besides, while such views are expressed, we are told that "the pneumonias which prevail in this country generally—sometimes sporadically and frequently as an epidemic—are really and substantially nothing more than a peculiar form of remittent and intermittent fevers," it must follow that such pneumonias are the result of the cause which produces these fevers, and which we may presume Dr. Merrill regards as different from that giving rise to what he holds to be the idiopathic form of the disease. Here, then, we find the same causes—atmospheric influences—officiating in some cases as the efficient and *sine qua non* agents in the production of the disease, and in others acting simply by promoting the development of the disease, the foundation of which had been laid by another and very different pathogenic influence. But, more than this; we find the same disease—for we are not informed of the existence of any symptomatological or pathological difference between the two supposed forms of pneumonia—ascribed, as already mentioned, to two distinct classes of causes; atmospheric vicissitudes on the one hand, and the peculiar agent, whatever this may be, which gives rise to periodic fever,

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on the other. The reader can scarcely fail to see that this mode of proceeding does not accord with the canon laws of that system of philosophy of which we hear so much, and by which it is to be hoped all medical inquirers are guided; for that philosophy teaches the impropriety of referring a specific effect to more than one cause. In the present instance, it would lead us to conclude that, since the idiopathic pneumonia of the south is admitted to arise from causes distinct from those that produce periodic or autumnal fever; since the same disease undoubtedly arises very frequently, not to say universally, from such causes in the north and elsewhere; and since, in the south, the agency of the former is essential to its manifestation, even when the causes of fevers are supposed to be instrumental in its production, there can be no reason for attributing pulmonary inflammation, when it appears in the southern section of this country, any more than when it shows itself in northern latitudes and in elevated positions, where autumnal fevers do not extend their sway, to a different morbific agent. Everywhere the same cause must produce the same effect, and a different effect must be produced by a different cause. Everywhere, therefore, the disease must arise from the same modifying agencies; and hence, if in some places it is due to atmospheric influences, there can be no propriety in lending our support to the idea that it arises elsewhere from the operation of agencies of a different kind. In other words, we must discard the theory which teaches that in the same place the disease occurs sometimes from the influence of one cause, at other times from that of another; and that the same influences—sudden transitions of temperature, and the like—act in some instances as an efficient cause, and in others lower themselves to the secondary office of exciting into action an inflammation produced by another agency.

Autumnal fevers, if not produced like other zymotic diseases, are localized in certain places; not so pneumonia.—Let us examine the subject in another point of view, and acknowledge, for a moment, that remittent, intermittent, and yellow fevers, and other zymotic diseases more or less allied to them, are not the offspring of peculiar morbid poisons exhaled from the localities where they prevail; and are, so far as that goes, on a par with pneumonic inflammations; I am not sure that the admission would afford much help to those whose opinions are under examination; for it is impossible to shut our eyes to the fact, that

the localization of those diseases takes place only where certain peculiar combinations of materials appertaining to the soil, or which have found their way there accidentally or otherwise, exist. Bearing this in mind, we arrive at once at the conclusion, that the real cause, whatsoever it may be, meets there certain agencies which so modify the system as to render it liable to their morbid impress. In a word, what many regard as the active and efficient cause of those diseases, may, after all, be only a predisposing agent. If this be correct, autumnal and periodic fevers are, in that respect, on an equal footing with other zymotic diseases arising from specific ferments or poisons. Every one knows that while Asiatic cholera and the febrile exanthemata are never produced by the malarial exhalations evolved from foul localities or marshy surfaces; while typhus and typhoid fevers are, as it is said, seldom the offspring of the former, and certainly never of the latter; while none of these diseases are occasioned by the ingestion of putrescent food, by the use of foul water, by imperfect ventilation, by starvation, by excessive muscular exertions, by the intemperate use of alcoholic liquors, and the like; and while, with the exception, perhaps, of typhus, they do not arise from the effluvia proceeding from the human body—particularly the lungs and skin—and consisting of the effete and highly putrescent matter mingled with the air or perspiration—it is a notorious fact, that they are principally rife in situations where such influences operate, and strike with greater violence, malignancy, and fatality among individuals exposed to their baneful effects. This is true, whether the disease be the product of a zymotic poison floating in the atmosphere, and independent for its development of any organic process, as Asiatic cholera; or whether it arises from a poison formed in the system and transmissible from one individual to another through means of contact, or the medium of the atmosphere; or whether, again, it is due to a particular poison proceeding from external sources of animal or vegetable decomposition, or from the result of a morbific condition of the system, as is the case, perhaps, with puerperal complaints, erysipelas, and *surgical fever.*

It will only be necessary to open the records of cholera in all the countries it has visited, and especially in England, where the subject has been investigated with greater attention than elsewhere, or of typhus or typhoid fevers, to be perfectly satisfied that, where the predisposing influences in question exist, there these diseases rage

with the greatest violence. Indeed, as Dr. Grainger well remarks, before the outbreak of any epidemic, knowing where the predisposing causes are rife, physicians can foretell the precise localities where it will occur; nay, even name the alley, or point to the exact house that will suffer.[1]

Now admitting, as already remarked, that autumnal fevers, of all possible grades, are to be placed in the same category with the diseases mentioned; admitting that the same relationship exists between them and the morbid influences above specified, as between these and the other forms of zymotic complaints; in other words, admitting, that not only intemperance, starvation, imperfect ventilation, putrescent food, foul water, but marsh miasmata and malaria generally, are restricted in their agency to the predisposing of the system to receive the morbid impressions of the efficient cause, it would not be the less true, that there exists a wide difference in that respect between those fevers and pneumonia.

We have already seen that the latter disease, so far from being necessarily more frequent in malarious countries, has often, if not generally, been noticed to prevail less extensively there than in places differently circumstanced. Malaria, therefore, cannot be regarded as its predisposing cause. If we turn to our wharves, courts, alleys, and the habitations of the poor, where sources of foul exhalations, of every kind imaginable, are abundant, we do not find that pneumonia is more generally noted there than in other localities. Nor do we find that putrescent food, foul water, imperfect ventilation, starvation, intemperance, or concentrated human effluvia, exercise an important influence in localizing the disease and increasing its prevalence. These morbid agencies, and a few others which predispose to fevers, may have an injurious effect as regards pneumonia; they may, by lowering the tone of the system, and diminishing the power of the latter to resist the force of the morbid impression, increase, to a certain extent, the danger of the attack; in the same way as do advanced age and other causes of constitutional prostration. But the injurious effect, so far as regards predisposition, is comparatively restricted in reference to pneumonia; while in regard to fevers and other zymotic diseases, it is strikingly great. Pneumonia attacks, and, the force of the constitution being the

[1] Influence of Noxious Effluvia on the Origin and Propagation of Epidemic Diseases. Quoted by Med. Exam. April, 1853, p. 253.

same, is equally fatal among all classes of society, the rich and the poor. We possess no authentic statements calculated to show that the latter are more prone to the disease than the former; for, if among the rich thoracic inflammations have occasionally been found to prevail less extensively, the explanation may be readily found in the fact that they are less exposed, not to the modifying influences above mentioned, which, as we have seen, predispose to fevers and zymotic complaints generally, but to the injurious impression of the ordinary exciting causes of the disease. In a word, autumnal and periodic fevers of all grades—supposing them not to be *produced* by malarial exhalations—are like other zymotic diseases, principally rife among individuals whose systems have been for a greater or less period of time under the depressing influence of such exhalations. These, like the rest of the recognized morbific influences mentioned, must, at the very least, be admitted, in accordance with that view of the matter, to act injuriously by predisposing the system to the action of the disease; and when examined attentively, the effect will be found to be evidently accomplished, as Dr. Carpenter well remarks, by producing in the blood of the individual exposed to them an excess of those decomposing organic compounds, which, as physiology teaches us, are always present in the circulating current, in minute proportion; being conveyed by it from the spots in which they are introduced, or in which they are generated, to the organs through which they are to be eliminated; and an excess of which is manifestly producible, either by the direct introduction of those matters from without, in the food or drink consumed, or in the air respired; or by the production of them within the body, at a rate beyond that at which they are normally eliminated; or by some obstacle to their elimination, which prevents the amount ordinarily originated from escaping at its normal rate through the usual outlets.[1] It would puzzle the most clairvoyant among the advocates of the hypothesis under examination, to discover and point out the existence of the most distant analogy in regard to the subject just mentioned, between pneumonia and autumnal fevers; for while the predisposing if not efficient causes of the latter are of the nature stated, and exercise their baneful influence in the way mentioned; while for this reason those fevers establish their claims to take rank among

[1] Brit. and For. Med.-Chir. Rev. Jan. 1853, p. 162.

zymotic diseases; pneumonia recognizes for its predisposing causes agencies of a very different character; such as, instead of lowering the purity of the blood, tend to increase its vitality, and would, in the absence of a thousand other reasons, suffice to put a bar to the idea of placing the disease among the zymotics, and establishing a parallel between it and fevers.

The opinion is incorrect, for it leads to the inference that the same cause produces diseases differing widely in symptoms and anatomical characters.—Nor does it appear less certain that the theory, or more properly the hypothesis, under examination, is faulty in another point of view; for while, as has been seen, it admits that the pneumonia of the south, if not pneumonia generally, is in some instances an idiopathic affection, and hence arises from causes distinct from those to which, it is thought, the disease should, in the majority of cases, be referred; in other words, while in accordance with that belief, inflammation of the lungs may be the offspring of two different sets of causes, we are given to understand that from the same cause—malaria, according to some, and atmospheric influences, agreeably to others—there may and do arise various series of morbid phenomena, which, when examined, are found to bear no resemblance to each other, and which, consequently, medical observers, from the days of Galen, if not of Hippocrates, to our own, have, with scarcely an exception, regarded as essentially dissimilar. What diseases, indeed, in the long list of those flesh is heir to, differ more widely from each other than pneumonia and malarial fever? Of the former, the seat is necessarily the substance of the lungs, with or without an implication of their covering and lining membrane, in which morbid changes are effected that need not to be specified here. While this is the case with these organs and tissues, the contents of the abdominal and other cavities may, and do often remain unaffected to the last; or if they become implicated at the outset, or during the course of the attack, the effect is generally due to causes independent of that which gave rise to the thoracic affection. When, in such cases, the inflammation of the lungs has been removed by proper means, or when, as not unfrequently happens—witness the effects of homœopathy—it subsides through the influence of the recuperative powers of the system, the fever by which it was accompanied disappears also, unless some other organ or tissue has taken on inflammation, and the latter keeps up febrile

excitement; or unless the case be complicated from the onset with some other complaint. This result could scarcely be looked for were the disease a constitutional one, caused by some general agency, and the local affection only a secondary effect superadded to the primary complaint. So far from this, the fever in pneumonia is dependent on and inseparable from the pulmonary inflammation, the removal of which is necessarily followed by the cessation of the former.

On the other hand, the febrile poison manifests an affinity for the abdominal viscera, which are affected to a greater or less extent in the large majority of cases. While doing this, it sometimes spreads its effects to the brain or its membranes; which, however, are more frequently involved in a secondary than in a primary way. But whatsoever be the morbid impression produced on these organs, the poison does not injure the pulmonary tissues, for which it has no affinity—so far at least as to excite in them the inflammatory change. Even on the abdominal organs, it does not always excite true inflammation; or when it does, the effect is felt by the mucous surfaces and secretory organs, and not by the serous and parenchymatous structures. If these at any time, and in any case, become the seat of inflammation, the result is due to fortuitous circumstances, and does not constitute an essential part of the disease, which may and does generally exist and run its course, to recovery or death, clothed in all its legitimate and characteristic colours, without exhibiting evidence of such an implication. Contrary to what occurs in pneumonia, when, in pure and uncomplicated cases of autumnal or periodic fevers, the physician succeeds in removing all the local inflammations which may exist, he must not be too sanguine about putting a stop to the fever itself; for the chances are a hundred to one that it will continue to run its course, uninfluenced in that respect by the cure of the local disease; in the same way that the alligator's head, in the experiments of a distinguished physiologist of our country, continued to live—saw, winked, snapped, and even jumped—though deprived of its usual appendage, the body.

Affinity of morbific and therapeutic agents for special organs not to be denied.—The fact of the affinity of deleterious agents for a particular part, is not one of which a well-read and observant physician can be ignorant; it has been observed and admitted from time immemorial. More than a century ago, Borden, whose authority in matters of the sort will not be impugned, remarked: "All morbific

miasms have their organs marked and predisposed for their germination. It is in these organs that each miasm penetrates; it is for them it has a special tendency; the herpetic attacks the skin, the scrofulous the glands, the venereal the organs of generation. The poison of gout affects the whole nervous system, and is only developed in the articular membranes. Each imparts to the individual in whom it germinates peculiar modifications, often of a diseased kind, often also consisting of a particular manner of being, or a well-characterized temperament."[1] At a later period, Mr. Hunter stated, in language no less explicit, that "poisons take their different seats in the body as if they were allotted to them." In a word, such agents, when introduced into or applied to the system, do not produce their effects on all the organs or tissues indiscriminately. So far from this, each morbid or common poison, each article producing some phenomenon which links it, as to its effects, with other articles, gives rise to phenomena of a special character, and is often found to occasion lesions of specific organs. While putrid substances, when inhaled into the lungs, or thrown into the circulation, affect the gastro-intestinal surface, the subcarbonate of soda applied in the latter way disorganizes the thoracic viscera. The lungs become gorged with blood, which gushes out when incisions are made into their substance, and bloody fluid is effused into the pleura, while the tissues and organs of the abdominal cavity are found in their normal state.[2] The lungs are specially affected by the bichlorate of mercury; the bronchiæ attract and eliminate phosphorus; the heart is affected by the upas antiar and digitalis; the kidneys by cantharides, oil of turpentine, indigo, saffron, and nitrate of potash; the liver by verdigris; the same or some other secretory organs are affected by mercury; the stomach by ipecacuanha, colchicum, and tartar emetic; the bowels and not the stomach by purgatives; the brain by opium; the spinal marrow and nervous system by nux vomica and prussic acid; the uterus by ergot; the capillary system by euphorbium, &c.[3]

The advocates of the malarial origin of pneumonia, some of whom seem to have forgotten this power of affinity, would do well to bear these facts in mind: as also the pustules of smallpox; the cynanche and erythema, and kidney affections of scarlatina; the intestinal

[1] Borden, Analyse Méd. du Sang. in Collected Works, 1011, 1012.
[2] Magendie on the Blood, 178, 183, 191, &c.
[3] See Blake, Sir Everard Home, Haller, Magendie, Legallois, Gaspard, Simon, &c.

ulcers of typhoid fever; the catarrh and eruption of measles; the rupia and periostitis of syphilis; the swollen parotid of mumps; the suppurating tumours of glanders; and other similar local affections which present themselves daily to our observation. Let them do so; and remember, at the same time, that "syphilis never produces ulcers in the ileum, scarlatina never causes iritis," and that "the causative poison of the one disease differs from the causative poison of the other, for on the selfsame subject it produces different effects,"[1] and they will, perhaps, feel less reluctance in admitting that the malarial poison exercises its specific effects on the mucous tissue of the gastro-intestinal apparatus, and on the spleen, and leaves the pulmonary organs uninjured.[2]

Distinctive symptoms and pathological conditions different in pneumonia and malarial fevers.—In all that precedes, I am borne out by the symptoms observed during life in the two diseases. In the former (pneumonia), the phenomena indicate the existence of acute inflammation, and greater or less congestion of the organ affected; while the blood exhibits changes, as well in regard to the proportion of its fibrin and other elements, as to its physical characters, which not only prove it to differ considerably from the healthy fluid, but assimilate the disease to a class very unlike the pyrexiæ. It need scarcely be remarked, that in all inflammations the blood is in what has been denominated a state of hyperinosis. It contains more fibrin than in the normal state—5 or 6 parts in 1,000, instead of 2 or 3—while the corpuscles decrease in proportion to the excess of the fibrin, from about 141.1 in 1,000 to about 128.0. The fatty matter also is increased; and, as a consequence of all these changes, the whole solid residue is diminished. The blood coagulates more slowly than in the normal state; the clot is not usually small, but very firm and consistent, and does not break up for a considerable

[1] Simon's Lectures on Gen. Path. 191.

[2] "If we were to attempt to explain how it happens that particular organs are affected rather than others, we must do so upon the most crude hypothesis. We must, therefore, as well observed by Mr. Piorry, accept the facts, without being able to account for them. Chemists cannot tell you why carbon has more affinity for oxygen than for azote; nor can physicians tell why cantharides, taken into the blood, act rather upon the kidneys than upon the brain. Whether we regard medicines or poisons, morbid secretions, excretions, contagions, or miasmata, absorbed into the blood, these observations are equally applicable."—*Ancell's Lectures on the Blood. Lancet*, 1840, p. 781.

time. It is almost invariably covered with a buffy coat, which is firm, tough, and intimately connected with the clot; its edge is often turned upwards and its surface uneven. If the clot be small, the buffy coat and the surface of the clot are more or less cupped, and the serum is of a pure lemon colour, not tinged red. When whipped, the fibrin separates in thicker and more solid masses than in ordinary blood. After the removal of the fibrin the corpuscles quickly sink, and frequently occupy only one-fourth of the whole fluid, while in healthy blood they sink very imperfectly, or not at all. The blood has always an alkaline reaction, and is of a higher temperature than in the ordinary state. All these changes are proportionate to the degree of the inflammation.[1] Such is the condition of the blood in the phlegmasiæ generally. In pneumonia, the state of hyperinosis is even more decided than in other diseases of that class, as the blood retains its heat longer; the clot is below the ordinary size, and very consistent, and does not break down for a considerable time. It admits of being sliced, and the sections retain their consistency for some time. Its surface is covered with the buffy coat, and is more or less cupped. The serum is of a pure yellow colour. The quantity of solid constituents is usually less than in healthy blood. The proportion of fibrin increases and varies, according to Andral and Gavarret, from 4 to 10.5 in 1,000 parts, with a mean (in 58 experiments) of 7.5; from 5.919 to 12.726, according to Rindskopf, and 3.4 to 9.15, with a mean of 6, according to Simon; while the blood-corpuscles, according to the first experimenters, amount to from 83.2 to 137, with a mean of 114.1; and, according to Simon, to as little as from 36 to 78.[2] If other symptoms present themselves, or if the blood assumes a different appearance, these results are due to complications; they do not constitute a necessary link in the chain of phenomena imparting an individuality to the disease, and are not, therefore, pathognomic.

Examine we now the distinctive symptoms of periodic and autumnal fevers—(applying to those terms their most enlarged sense)—those symptoms which alone enable us to diagnose those fevers—which impart to them their individuality, and assign them an independent position in every nosological arrangement, and we shall not discover one calculated to lead us, for a moment, to the

[1] Geo. Polli, Richerche et Esperienze, sul sangue umano Annali Universali, cxiii. 333; Simon, Animal Chem. i. 250; T. W. Jones, Edinb. Journ. lxxvi. 108.

[2] Simon, *op. cit.* i. 258, 259, 262.

supposition that the lungs are the seat of inflammation or acute congestion. So far from it, indeed, respiration is less impaired in the early stage of those diseases than most of the other great functions. When it becomes deranged, the effect is of a nervous character, or the result of the inability of the blood-corpuscles to carry oxygen; and physical exploration usually indicates no important morbid change in the pulmonary tissue, and when it does—as more frequently occurs during the latter stage of malignant cases—the signs are those of passive congestion. Bronchitis, except in a very few cases, is not developed, and when so, is usually traced to its legitimate causes, and need not be laid to the score of the malarial poison. At the same time, all the symptoms observed —all, I repeat, that are pathognomonic—point most usually to the stomach, bowels, liver, and spleen, sometimes to the brain, as being the organs principally implicated. Sometimes they indicate the existence of acute inflammation in one or more of these—more generally of simple congestion, and more frequently still of functional disorder of the former, with or without congestion of the latter—of the spleen particularly—which in some of the pyrexiæ bears a large share of the brunt of the disease. The blood at the same time either exhibits, as in some of the milder forms of the disease —simple intermittents, for example—or in the early stages of other forms of a more severe character, little or no change from its normal state; or manifests in the more advanced stage of these, and even in some cases of intermittents, as well as in the early stages of the other, and particularly in the malignant grades, alterations in its chemical composition and physical characters the very reverse of those observed in pneumonia and the phlegmasiæ generally. The fluid is in that state which has received the denomination of hypinosis. The fibrin is frequently less than in healthy blood, or if it amounts to the normal quantity, its proportion to the blood-corpuscles is less than is found in a state of health (2.1 : 110, Simon, or 3 : 110, Lecanu); the quantity of corpuscles is either absolutely increased, or their proportion to the fibrin is larger than in the healthy state; and, in addition, the quantity of solid constituents is also frequently larger than in the normal fluid.

The clot, though sometimes small, is more commonly the reverse. It is soft, diffluent, and of a dark, almost red colour. Occasionally, indeed, no clot is formed. The buffy coat is seldom seen, and when this is the case, is thin and soft, and forms a gelatinous parti-coloured

deposit on the clot. The serum is sometimes of a deep yellow tinge from the colouring matter of the bile; or red, from blood-red corpuscles in suspension. The blood has always an alkaline reaction.[1] In some forms of the disease, it even approaches that state denominated spanæmia; it is watery, very poor in fibrin, and of a dark colour. If any clot be formed, it is diffluent and very soft, the serum is frequently of a dark yellow or brown-red colour; partly from the colouring matter of the bile, and partly from dissolved hæmatoglobulin. It possesses a very peculiar smell, which not improbably depends on a volatile salt of ammonia.[2] These changes, which can be artificially produced by the introduction of putrid substances into the circulation, are observed in a more or less marked manner, not only in malarial remittents, sometimes in intermittents, but also in typhoid, typhus, relapsing, yellow, and pestilential fevers, as well as in a variety of other diseases arising from the action of morbid poisons.

The fevers of this country, from the simple intermittent to the malignant yellow, have exhibited them to a greater or less extent. As regards the latter form of the disease, the altered condition of the blood has been noticed by Lining and Moultrie, and, since them, by Rush, Deveze, Nassy, Physick, Cathrall, Brown, Currie, Caldwell, Miller, Drysdale, E. H. Smith, Potter, Gros, Cartwright, Kelly, Nott, S. Jackson, Stone, and by almost every other writer on the subject. It is, indeed, a fact of so notorious a character, as not to call for any special illustration in this place. Not less evident, though perhaps not so distinctly marked, are the changes in question on the milder forms of the disease. They were noticed by Dr. John Mitchell, in the fever he observed in Virginia in 1737, 1741, 42, and which was improperly denominated yellow fever.[3] Since then, attention has been called to them in a more or less pointed manner by Drs. Daniels, Dickson, Drake, Stewardson, and other American writers, as also by Dr. Stevens, who had an oppor-

[1] Simon, Animal Chemistry, i. 287, 301, 302.

[2] *Ibid.* i. 319; *ibid.* Appendix, ii. 510.

Salvagnoli examined the blood of four persons actually labouring under, or who had just recovered from intermittent fever, and were living in a malarious district. He found that it exhibited a notable diminution of albumen and fatty matter, and that the phosphates had almost entirely disappeared. It contained, however, a large quantity of cholesterin. It is remarked that the biliary secretion of those living in such districts has been previously noticed to be rich in cholesterin. Saggio illustrativo della Statistica Medicà della Maremme Toscane, 66.

[3] Med. and Philos. Register, iv. 188.

tunity, during his visit to this country, to observe our lake and marsh fevers. It would be useless to enter at large on the subject; and I shall content myself with making room only for a brief summary of observations made on the subject by Dr. Frick of Baltimore. This writer examined twelve cases of autumnal fever. Of these, five were of the remittent, and the balance of the intermittent type. In two of those cases—one of the intermittents, and one of the remittents, the disease assumed the congestive form. In four classed as remittents, the proportion of the fibrin was above the usual standard. In the fifth case of that class, the disease was of three weeks' duration, and all the elements of the blood, except the chlorides and the phosphates, were below their natural standard. In five of the cases of intermittent fever out of seven, the fibrin was below the average quantity. Of the two exceptional cases, one was complicated with ascites, and œdema of the lower extremities, coming on as an acute affection, and being preceded by a chill; and the other was complicated with pneumonia at the summit of the lung. The increase of fibrine in the four cases of remittents, as above mentioned, was due to the superaddition of gastro-duodenitis to the original disease. The blood-globules in the remittent form were, as is the case in the pyrexia generally, in all the instances except the one above alluded to, increased.[1]

The same changes have been noticed not only as regards typhus, typhoid, and relapsing fevers, but common malarial diseases, in England, France, Ireland, Scotland, Italy, and Germany, as well as in the West Indies, Algeria, Western Africa, &c., as the reader may readily find by referring to the writings of Huxham, Cleghorn, Sarcone, R. Armstrong, Evans, Tweedie, Cormack, Stevens, Boyle, J. Davy, Andral and Gavarret, Leonard and Foley, Clanny, Burne, Stokes, Reid, Stoker, Haspel, Boudin, McWilliams, Jennings, J. Armstrong, Lecanu, Cozzi, Salvagnoli, F. Home, and Clot-Bey, to say nothing of the very large majority of authors who have treated of the yellow fever of Spain and tropical regions.

In the same way that in pneumonia we find that a condition of blood, more or less approximating to the one mentioned above, indicates the complication of a typhoid diathesis, or of a strong malarial taint, so in fevers arising from the latter cause, or in the pyrexiæ

[1] Of the Relative Proportions of the different Organic and Inorganic Elements of the Blood in different Diseases, Am. J. xv. (N. S.) 29.

generally, the excess of fibrin, and its greater proportion relatively to the blood-corpuscles, indicates the existence of a phlegmasia in addition to, but forming no essential part of, the disease.

These changes in the appearance of the blood are noticed not only during the attack of malarial fever; they have been observed even in individuals in apparent health residing in malarial regions, and especially during the prevalence of an epidemic of remittent, intermittent, or yellow fever; and, indeed, of many other forms of zymotic diseases. The late Dr. John Mitchell, of Virginia, in the account he left us of the so-called yellow fever which prevailed in some parts of that State about the middle of the last century, remarks: "Even those who are bled after a received contagion before the fever is formed, have a thin, dissolved, florid blood, even in winter. This was the constant state of the blood in about thirty or forty whom I have known to have been bled at all seasons of the year."[1] Alluding to the fever of Baltimore in 1800, Dr. Potter says: "To ascertain the appearance of the blood in subjects apparently in good health, I drew it from five persons who had lived, during the whole season, in the most infected parts of the city, who were, in every external appearance and inward feeling, in perfect health. The appearance of the blood could not be distinguished from that of those who laboured under the most inveterate grades of the disease. A young gentleman having returned from the western part of Pennsylvania, on the 10th of September, in good health, I drew a few ounces of blood from a vein on that day; it discovered no deviation from that of other healthy persons. He remained in my family till the 26th of the month, and, on that day, I repeated the bloodletting. The serum had assumed a deep yellow hue, and a copious precipitate of red globules had fallen to the bottom of the receiving vessel."[2] Dr. Archer, of Norfolk, has also noticed the same fact, stating, as an evidence of the extent to which the predisposing causes of this fever operated, that the blood taken from healthy persons generally exhibited changes which were easily discernible as it trickled down the sides of the basin. These were pretty regularly increased as you approached the infected district.[3]

The same fact is confirmed in regard to malarial fevers of lower grades by the results of observations made in the Tuscan Maremma

[1] Med. and Philos. Reg. iv. 188. [2] A Memoir on Contagion, 54.
[3] Hist. of the Yel. Fev. of Norfolk in 1821, Med. Rec. v. 68.

and in the miasmatic districts of this country. "So great and so constant is the difference," says a high authority in reference to the former, "that, from the physical examination of the blood only, almost without error, the physician may judge if the patient resides constantly in an unhealthy atmosphere, and if the liver and spleen have been altered."[1] During his visit to the United States, Dr. Stevens saw much of our lake fever, especially in the Genesee country. In the months of September and October, 1830—the sickly period—he bled several individuals who resided in some of the infected localities, but had not yet been attacked with the fever. In every one of these, the blood invariably presented the same diseased appearance which he had often observed in those who resided near to swampy situations in the West Indies. It was very dark in colour, and evidently deranged in its physical appearance, while the serum which separated, in place of being clear, had a muddy or brown colour, and, in some cases, an oily appearance. In fact, Dr. Stevens did not meet with even one intelligent practitioner in that country, who was not aware of the fact that the blood of the inhabitants, during the sickly months, is very different from that of those individuals who arrive from healthy situations; whilst even in those who reside in the most unhealthy situations, and who have not yet had the fever, it is not only dark in colour, but evidently so much diseased in its properties as to be very unlike the blood in health.[2]

To this it may be added that, in pneumonia, the blood preserves its ordinary and specific odorous exhalation; while in diseases arising from the action of the malarial poison—in some forms of them at least—the fluid acquires a peculiar odour not encountered in other complaints. In some forms of fever—typhus, plague, and yellow fever—the blood possesses a very peculiar smell, which probably differs in each disease, and is produced by a volatile salt of ammonia.[3] Dr. Stevens, in speaking of the condition of the blood in what he calls the African typhus, remarks that, "when first drawn, it has a peculiar smell."[4] And Dr. Rush, in his account of the epidemic of 1794, mentions that a similar statement was made to him by a German bleeder, whose experience in 1793 was very ample, and affirmed he could distinguish a yellow fever from all other forms of fever. "From the certainty of his decision in one

[1] Saggio illustrativo le Tavole della Statistica Medicà della Maremme Toscane, 211.

[2] On the Blood, 216.

[3] Simon, *op. cit.* i. 319, 320.

[4] On the Blood, 219.

case which came under my notice," adds Dr. Rush, "before a suspicion had taken place of the fever being in the city, I am disposed to believe that there is a foundation for his remark."[1] I may add, that Mr. Ripperger, a highly respectable bleeder of this city, who has gone through all our epidemics from 1793 to the present day, and, during that time—thanks to the system long in vogue—shed more blood than any ten men living or dead, has confirmed to me the above statement of Dr. Rush's authority. And why should it not be so? It is known, particularly since the experiments of Barruel, that the blood of every vertebrate animal has in it an odoriferous principle, identical in all the individuals of the same species, and similar to the odour of the cutaneous transpiration, or to that part of it which gives to each animal its characteristic smell. We know, also, that according to the principle laid down by Barruel, and more or less acknowledged by other chemists, that the blood of each individual exhales an odour closely resembling that of the cutaneous perspiration, and so peculiar that the species, and even the sex of any animal from whom a given quantity of blood has been drawn, may be determined by it.[2] It is known, also, that certain poisonous and other substances—ether, hydrocyanic acid, camphor, and alcohol, which, when taken into the system, find their way into the circulation, impart peculiar odours to the blood; and when we combine these various circumstances together, we cannot help perceiving that the malarial poison, when absorbed, may so modify that fluid as to produce analogous effects.

Are we not justified in explaining, by the changes which occur in the blood of individuals affected with autumnal and other fevers, many of the phenomena which present themselves in the course of those diseases? On the subject of anæmia and scorbutic attacks, there can be but little difficulty. In reference to intermittents, a writer in the *Dublin Journal* (vii. 219) says, and his remarks apply equally well to other fevers: "The diminution of the globules (probably also a diminution of their power of absorbing oxygen) explains the prostration of the entire constitution and the occasional disturbances of the circulation. The diminution of the fibrin explains the ecchymosis on the skin and in the cellular tissues, the bleeding from the nose and gums, the gangrene of the mouth which sometimes occurs, and the pain in the limbs. The diminution of

[1] Works, iii. 223.

[2] Brit. and For. Rev. xi. 226.

the albumen explains the hydropsical swellings, anasarca, ascites, and probably also the watery diarrhœa, which always closes the last scenes of persons weakened by fever."

Another writer remarks: "The first action of the poison is apparently upon the functions of the blood; those are impaired, or, in peracute cases, cease altogether. The functions of the blood are, first, to maintain the activity of the nervous and muscular systems; and, secondly, to supply the materials for the molecular changes constantly going on in the tissues. It is essential to this being properly performed that the blood-corpuscles be in a fit condition to carry oxygen, and it would appear from the symptoms which mark every stage of fever, that this function of the corpuscles is impaired from the first. This is indicated by the *besoin de respirer* developed in the premonitory stage; the sensation and condition of the respiratory organs are precisely the same as if their capacity had been diminished, and due aeration of the blood thus prevented. The patient takes many forced inspirations, sighs, or gasps, and the breathing is quickened on the least exertion. The hæmato-globuline is changed also, for the skin assumes a peculiarly pale, sallow, and unhealthy look. The function of the nervous system is impaired in consequence of these changes in the blood, namely, the changes effected by the poison and the defective oxygenation—hence lassitude and weariness, disturbed functions, or congestion of some or all of the viscera, and a lower temperature."[1]

From all that precedes, it follows that in uncomplicated pneumonia, we have febrile reaction attended necessarily with, and depending on, local inflammation of the lungs—which fever is accompanied with an increase of fibrin in the blood. In the autumnal (as also in other) pyrexiæ we have likewise fever; but unlike what occurs in the former, this fever is not necessarily attended with symptoms of local inflammation, and is characterized by a defibrinated condition of the blood. This defibrinated condition of the blood, and the symptoms accompanying it, many of which it serves to explain, are analogous to, or resemble, those produced by the introduction of putrid or poisonous matters into the circulation; whilst the opposite condition of the same fluid in pneumonia serves to explain the symptoms by which this disease is attended, and would be totally incapable of accounting for those that are characteristic

[1] Brit. and For. Med.-Chir. Rev. iii. 95, 96.

of idiopathic fever; at the same time that it could not in any contingency be the result of a poisonous agent thrown into the circulation. Can any one, with such facts before him, seeing an increase of fibrin on the one hand, and a diminution of the same element on the other, unite in sentiment with those who regard the two diseases as pathologically identical, and maintain with them that pneumonia is really and substantially nothing more than a peculiar form of remittent and intermittent fever? In other words, can he be persuaded that a peculiar disease, characterized by a particular condition of the blood, and depending for its existence on the local inflammation of a special organ, is only a different form of another disease, marked by a diametrically opposite condition of that fluid, and not necessarily connected with the inflammation of that, or any other organ?

Odour of the surface peculiar in some malarial diseases; not so in pneumonia.—Nor do we find that the two diseases approximate much more closely in respect to the odour emitted by the sick. In pneumonia, as in other kindred inflammations, the odour, apart from that which accompanies ordinary perspiration consequent on, or occurring during or at the close of the stage of febrile excitement—more frequently at the period of crisis—or from that which depends on the peculiar complexion of the patient, or the particular idiosyncrasy he may possess, presents nothing unusual, and appertaining in a special manner to the disease;—nothing which is not seen every day in other and dissimilar febrile complaints—nothing which may serve to impart to the case a particular pathogenic character. If this does not hold good in all cases, the exceptions occur principally in the typhoid form of the disease, or when the case, though not originally of that kind, falls into a low malignant condition; or they occur when the pneumonia is complicated *ab initio* with some disease in which the phenomenon commonly shows itself; but, in such instances, the fetid odour emitted belongs to that typhoid condition, or to the other complaint superadded to the thoracic inflammation, and not to the latter itself. If, now, we pass from this disease to the malarial pyrexiæ, we shall find that in these, particularly in the advanced stages, and in the malignant forms, the odour of the surface constitutes frequently a prominent, and, as it were, a characteristic symptom, which, considering the connection between the odour of the blood and that of the skin in the state of health, may

be presumed to arise from that imparted to the circulating fluid by the malarial poison. We may even go so far as to aver, that each variety of malarial fevers presents something peculiar in that respect, which, while enabling the pathologist to distinguish it from pneumonia and other true phlegmasiæ, affords him the means of pointing out, to a certain extent, at least, the particular nature of the case under examination, or the class to which it belongs. Speaking of the second period of the Niger fever, Dr. McWilliams says: "The principal complaint at this period was from the odour of the perspiration, particularly in those cases that subsequently proved fatal. I was not sensible of this peculiarity in the smell of the perspiration in my own case, but I perceived it very distinctly in several others."[1] In the bilious remittent fevers of Ceylon and the West Indies, a peculiar smell emitted by the sick is alluded to by Dr. Millingen, and other writers.[2]

In the yellow fever, a peculiar odour is described as being of still more common occurrence. I am aware that many writers on the disease as it shows itself in tropical and temperate regions, have taken no notice of this symptom; and that there are not wanting those who have expressed doubts as to its occurrence, or even denied it altogether. Dr. Townsend, for example, in his account of the epidemic of New York in 1822, says that there was not, at any time, as far as his experience goes, a foul cadaverous effluvium from the body, "a symptom so peculiarly characteristic of typhus, with which yellow fever, however, ought not to be confounded. On the contrary, in a majority of cases, everything about the patient seemed perfectly inodorous, and for hours after death."[3] Dr. Tully also, in speaking of the fever of Middletown, Conn., in 1820, informs us that, in general, there was but little fetor before death, and even the usual febrile smell was not, in many instances, perceptible.[4]

But whatever may have been the case in New York and Middletown, at the periods in question, and in other instances that might, if necessary, be mentioned, and however true it may be that from these facts and the silence of several writers on the subject, we may infer that the surface of the body in the yellow fever does not

[1] *Op. cit.* 133.

[2] Second Report of London Board of Health, 364; James Clark, Fever of Dominica, 20.

[3] *Op. cit.* 165.

[4] Essays on Fevers and other Medical Subjects, &c. 30.

invariably emit an odour of a peculiar kind; yet facts sufficient may be gathered from the writings on this disease, in both temperate and tropical regions, to bear me out in what is here stated, and to show, not only that the symptom in question occurs often, but does so under circumstances calculated to induce us to view it as pathognomonic of the fever generally. I cannot doubt having recognized a peculiar and offensive odour about the persons of individuals affected with this disease; and I think it has enabled me, in some instances, to establish a correct diagnosis. In one or two cases the smell was highly offensive. It has been noticed in all our epidemics. Dr. Deveze[1] speaks of it as of frequent occurrence in the latter stages of the disease, and designates it as fetid—*une odeur infecte.* Dr. Rush[2] states that, in 1793, the sweats in some persons had an offensive smell resembling that of the washings of a gun. This odour was emitted by those who, though not ill of the fever, were exposed to the prevailing cause. It was perceived even in those cases in which the disease went off on the first day of the attack.[3] In his history of the epidemic of 1794, the same eminent physician says: "I recollect having more than once perceived a smell which had been familiar to me during the prevalence of the yellow fever in 1793. It resembled the smell of liver of sulphur. I suspected for awhile that it arose from the exhalations of the gutters of the city. But an accident taught me that it was produced by the perspiration of my body. Upon rubbing my hands, this odour was increased so as to become not only more sensible to myself, but in the most sensible degree to my pupil, Mr. Otto. From this fact, I was convinced that I was strongly impregnated with miasmata."[4] Dr. S. Jackson mentions that in one case a most intolerable fetor proceeded from the patient's body for twenty-four hours before his entire and complete dissolution.[5] Drysdale,[6] in Baltimore; Hill,[7] in Wilmington, North Carolina; Gros,[8] in New Orleans; Stone,[9] in Woodville and Natchez; Dr. A. Hosack, in New York;[10] Dr. Manson,[11] in New Haven, have made similar observations during the epidemics they describe.

[1] Traité de la F. J. 26.
[2] *Op. cit.* iii. 85.
[3] *Ibid.* iii. 63.
[4] *Ibid.* iii. 217.
[5] Fever of 1820, p. 54.
[6] Med. Mus. i. 137.
[7] Med. Recorder, v. 90.
[8] Rep. on Fever of 1817, N. O. 12.
[9] Y. F. of New York in 1795, p. 16.
[10] Webster's Collection, 182.
[11] N. O. J. ii. 180; *ibid.* v. 453.

Speaking of the fever of Woodville (1844), Dr. Stone remarks: "Sometimes before, and always after an attack, and not dependent upon the perspiration, a peculiar odour was perceptible, which it is not easy to describe; but which to have observed once is to remember always; I think I could detect the disease by this alone. One of my children, aged eight years, gave off this odour twelve days before the development of fever." Besides this, the perspiration was of an offensive odour. Of the symptom under present consideration, as it presented itself four years after in Natchez, the same writer says: "An odour which I cannot describe, but which was *the* odour that had been indelibly impressed on me in 1844, was strongly perceptible in many of the simplest cases towards the latter part of the epidemic, but which I did not notice for a month after its commencement." On this, as on the former occasion, the "perspiration was always offensive, often fetid."

In the yellow fever epidemics of Leghorn, and of various parts of the South of Spain, the same symptom was frequently observed, and is specially noticed in the numerous accounts of them which we possess. In the first-named city, the fetor was compared to that of fetid bile.[1] Arejula,[2] at Malaga, in 1803, likened it to the smell of putrid fish. It is mentioned by the same writer, Berthe,[3] and others, as of common occurrence during the epidemics of Andalusia, from 1800, and 1810. Martel,[4] in describing the disease as it occurred among the French soldiers in 1811, at Rotana, Levrilla, and Alcantarilla, lays great stress on that symptom. Dr. R. Jackson[5] informs us that, at Cadiz and Xeres, in 1820, the fetor emitted by the body of the sick, in some forms of the disease, was singular—sickly and faint, and not unlike the smell of a fish-market. The next year, at Barcelona, Pariset[6] and his colleagues noticed a cadaverous odour in some, though not generally. Audouard[7] mentions it as of frequent occurrence; so does Lafuente,[8] and other Spanish writers. Rochoux[9] states that some of the sick emit a very offensive odour, analogous to that of gangrene; and adds that it is a usual attendant on the cutaneous exhala-

[1] Edinb. Journ. ii. 84.
[2] *Ibid.* i. 448.
[3] *Op. cit.* 55, 56.
[4] Quoted by Bally, 250.
[5] Epid. Y. F. of the South Coast of Spain, 105.
[6] *Op. cit.* 435.
[7] *Op. cit.* 211, 393.
[8] Observaciones sobre la Fiebre Amarilla, &c. Periodico de la Soc. Med. Cir. i. 165. (See Rochoux, 472.)
[9] Rochoux, *op. cit.* 461, 472, 473.

tion, whether the latter assumes the character of sweat or of insensible perspiration. "It is not," he adds, "so strong as to be perceived from the street, as some physicians have asserted; but we never fail to be struck with it when we uncover a patient and approach very near him. It is of an insipid nature (*fade*), somewhat nauseous, and adheres strongly to clothes." The same writer remarks, in addition, that, on this point, the yellow fever approximates closely to hospital typhus, and the plague, both of which are characterized by a peculiar odour.

Nor is this all; Desportes,[1] more than a century ago, noticed the same phenomenon in the fever of St. Domingo, where it was subsequently observed by Bally[2] and Gilbert,[3] the former designating it as fetid, the latter as cadaverous. Dr. R. Jackson[4] found in the West Indies, as he did in the fever of Europe, cases in which the perspiration had a peculiar smell, resembling that of a fish-market. In the fever of Dominica, as we learn from Dr. Imray,[5] the odour of the cutaneous exhalation was often extremely disagreeable as well to the patient himself as his attendants. Additional testimony on this subject might be gathered from the writings of Gillespie (34), Savaresi (273, 4), Madrid (25), Vatable (346), Frost (*Med. Repos.* xiii. 33), Comrie (*Ed. J.* xiii. 177), Ralph (*Ed. Med.-Chir. Tr.* ii. 75), Arnold (10).

The condition of the urine different in the two diseases.—It may not be improper to call attention, in this place, to the condition of the urine in the two diseases. As is known, this fluid in its normal state, contains, on an average, 11.88 of urea, 0.395 of uric acid, 6.80 of inorganic salts, and 8.60 of organic matter. Of the salts, the chloride of sodium may be estimated at from $\frac{3\cdot600}{1000}$ parts to $\frac{4\cdot45}{100}$ in 1,000. Now when, with this before us, we inquire how matters stand in respect to malarial fevers and pneumonia, we find that these diseases differ in no inconsiderable degree from each other and from the state of health, so far as the proportionate quantity of those ingredients is concerned. In the former the urea is in less quantity than in health, presenting a proportion of only 9.01. At the same time, the quantity of uric acid attains an amount of 9.80, or eight times larger than in health. As regards the inorganic

[1] Maladies de St. Domingues, i. 221. [2] Typhus d'Amérique, 247, 248.
[3] Hist. Méd. de l'Armée Française à St. Domingue, 66.
[4] Sketch, i. 64. [5] Edinb. Med. and Surg. Journ. liii. 80.

salts, we find them to rise as high as about 16.72, or much more than twice as high as in healthy urine, while the organic matter varies but slightly, if at all, from the normal state. As a matter of course, amid this increase in the salts, the chloride of sodium can suffer no diminution, and in all probability has increased in the same ratio as the other salts.[1] During the stage of apyrexia in intermittent fever, the urine is often found, so far as the above ingredients are concerned, in its natural state; a condition proportioned in great measure to the extended duration of that stage and the shortness of the febrile paroxysm. In some cases the fluid is healthy throughout. But in most instances the changes under consideration exist as well in the intermissions as during the paroxysms, and are greater in proportion to the length of the disease. In no instance can we discover that the proportion of uric acid remains at the normal point, and that the quantity of the fixed salts falls considerably below.

In reference to the phlegmasiæ, and pneumonia in particular, the proportions are nearly reversed; for Simon and other high chemical authorities have shown that the urine therein exhibits a marked diminution of the inorganic salts, and a greater relative amount of organic constituents; the proportion of the ingredients mentioned being urea, 7.3; uric acid, 0.4; fixed salts, 2.7; and organic matter, 8.8.[2] In an examination by Becquerel (*op. cit.* 329), the following proportions, which differ but little from the preceding, were obtained: urea, 7.761; uric acid, 0.464; inorganic salts, 2.871; organic matter, 9.332. Of the salts, the chloride of sodium was early found to suffer a greater diminution than the others in all the phlegmasiæ. Subsequent observations, made some three years ago by Dr. Redtenbacher, leave no doubt that, in pneumonia, the result in question is still more marked and constant than in inflammation of other organs; for while in these the chloride is usually only greatly diminished, and sometimes, indeed, preserves its normal proportions, in pneumonia the quantity of the salt never fails to diminish gradually until the period of hepatization has occurred, when no traces of it can be detected in the urine, and only makes its appearance again as the resolution of the inflammation proceeds. It may be remarked that this effect cannot be due solely to the altered diet

[1] Becquerel, Séméïotique des Urines, 286–291; Heretier, Chimie Méd. 528; Simon, *op. cit.* ii. 255–257.

[2] Simon, *op. cit.* ii. 216.

taken by patients labouring under the disease, for other inflammations require the same diet, and we have just seen that in them the phenomenon often fails to present itself.[1]

By Mr. Beale, of London, the subject has been recently taken up and apparently examined with considerable care; and from his researches in the matter, he arrives at the following conclusions:—

1. That in pneumonia, there is a total absence of chloride of sodium from the urine, at or about the period of hepatization of the lung.

2. That soon after the resolution of the inflammation, the chloride becomes restored to the urine, and often in considerable quantity.

3. That at this period the serum of the blood is found to contain a greater amount of chloride than in health.

4. That the presence of chloride of sodium in the urine may be taken as evidence of the existence of a greater quantity of the salt in the blood, than is required for the wants of the system generally, or at least of an amount sufficient for that purpose; and that the absence of the salt from the urine indicates that the circulating fluid contains less than the normal quantity.

5. That the sputa in pneumonia contain a greater quantity of fixed chloride than healthy pulmonary mucus, if there be not much less than a normal amount in the blood, although there be a complete absence of the salt from the urine. In all cases, however, there is found in the sputa a quantity many times greater than exists in an equal amount of blood at the same period of the disease. The absolute amount present is subject to variation at different periods of the disease, and in different cases.

6. That there is reason to believe that the absence of the chloride of sodium from the urine during the stage of hepatization, depends upon a determination of this salt to the inflamed lung, and that, when resolution occurs, this force of attraction ceases, and whatever salt has been retained in the lung is reabsorbed, and appears in the urine in the usual way.[2]

Here, then, we perceive a strong contrast existing between pneumonia and malarial fevers—increase of urea in the one, and diminution of the same in the other disease; diminution of uric acid in the former, and great increase in the latter; diminution of the salts in the one, great increase of them in the other; considerable diminution, and finally total absence of the chloride of sodium

[1] Edinb. J. lxxx. 246. [2] Med.-Chir. Trans. xxxv. 355, &c.

in the one, increased or normal quantity of the same in the other disease; finally, vast increase of organic matter in the former, ordinary quantity of it in the latter. To this, let it be added that when in malarial fevers results of an opposite nature are obtained, they will be found due to an inflammatory complication.

But I have sufficiently enlarged upon these topics. Nor shall I do more than refer to the peculiar acid alliaceous odour of the breath, which has been regarded, justly no doubt, as among the characteristics of yellow fever;[1] the pearly appearance of the gums in the same disease, to which attention was first called by Dr. Valetti, of New Orleans; to the peculiar indentation at the roots of the nails, "indicating the point at which their growth was suspended by the disease;" observed, it is said, in 1847, by Dr. Walkly, of Mobile, in all the forty-one cases he examined;[2] or to the bluish discoloration of the gums said to be peculiar to all malarial diseases. These subjects have not yet been examined to a sufficient extent to be used in this place for purposes of comparison.

Anatomical characters different in the two diseases.—Nor is the discrepancy less marked in regard to the anatomical characters of the two diseases. In the one, there is often no traces of inflammation, properly so called. When these do present themselves, they are seated in the gastro-intestinal surface; sometimes, though not always, in the cerebral organs and membranes, and occasionally in the liver. As frequently, these organs are only congested. Usually, the liver is neither inflamed nor greatly congested, being in some forms of fever (the bilious remittent) of a bronze hue, depending on the deposit of a dark melænic pigment, not unlike the dark solid substance of black vomit. It does not contain a larger quantity than natural of fatty matter, but is filled with bile.[3] In another form of malarial fever—the yellow—the organ is of a pale yellow colour, and dry anemic texture. It bears the marks of having secreted little or no bile during the course of the attack,[4] and has been shown by Dr. A. Clark, of New York, whose discovery was fully

[1] Kelly, Am. J. xiv. (N. S.) 374. [2] N. O. J. v. 481.

[3] Brit. and For. Med.-Ch. Rev. Jan. 1849, pp. 95, 96. Stewardson, Am. J. (N. S.), i. 313; Swett, *ib.* iii. 33, 35; Anderson and Frick, *ib.* xi. 332; Anderson (of Alabama), Prize Essay, Proceedings of Alabama Medical Association, 1852, p. 117.

[4] See Louis on Yellow Fever, and many other works on the same disease.

confirmed during the last autumn in Philadelphia, to contain, and to owe its peculiar hue, to the deposit in the hepatic cells of a large quantity of oily or fatty matter, which, in some cases, imparts to the organ much of the characteristic appearances of a common fatty liver. The spleen is usually found congested, softened, or enlarged, or even hypertrophied. This condition of the organ appertains more specially to the intermittent and remittent forms of malarial fevers, and has been noted universally in this country, in France, England, Holland, Algeria, India, and, indeed, in every country where these diseases prevail, and where dissections have been performed. It is noted also in typhoid, and in the relapsing fever of the British islands—so frequently, indeed, as to be viewed in the light of a characteristic phenomenon.[1] Finally, the blood in general is found altered in colour, often more or less fluid, and exhibiting, in malignant cases, signs of decomposition. But amid all these changes, the lungs remain unimplicated; or, if otherwise, the occurrence is rare, and differs in nothing from what is seen in other acute diseases not especially seated in those organs.

Here, then, we perceive that neither this affection, nor any local inflammation, nor a buffy or cupped blood form part and parcel of autumnal fevers during life, or leave traces after death. In the other disease, on the contrary—pneumonia—the lungs never fail to present marks of inflammation, varying in character according to the stage at which the disease had arrived; and while such changes are necessarily and invariably detected in those organs, the stomach, bowels, liver, and spleen are very usually found in their normal state. In fact, no sign observed during life, no anatomical character discovered after death, can be adduced in evidence of the pathological identity of those diseases, or induce us, for a moment, to think they arise from the same cause. In the one case, the latter consists

[1] Cleghorn, 84 (5th ed.); Monfalcon, 298, 532; Addison, Lond. Med. Gaz. xxiii. 796; Forbes's Rev. xviii. 189; Tweedy, 33, Am. ed.; Copland, ii. 1089; Henderson, Edinb. J. lix.; Dickson, Charleston J. i. 20; Am. J. July, 1852; Drake, i. 830, 842; Swett, Am. J. (N. S.) iii. 33, 35; Stewardson, Am. J. (N. S.) i. 314; Bartlett, 332; Anderson and Frick, Am. J. (N. S.) xi. 332; Haspel, Mal. de l'Algérie, ii. 201, 317; J. Davy, ii. 236; Williams, ii. 470; R. Jackson, i. 80; Boudin, Géographia Méd. 46; Cycl. of Pract. Med. ii. 223; Twining, Fevers of Bengal; Boyle, Dis. of West C. of Africa, 89, 141, 146; Durand, Mem. &c., sur les rates engorgées pendant les f. Interm. 6; Bryson, 73; Evans, 221; Marshall, Top. and Dis. of Ceylon, 142; Watson, 455; Anderson, *loc. cit.* 117; Nepple, F. I. 61, 270; Audouard, Bulletin, xii. 151; C. Broussais, *ib.* 293; Corney, *ib.* 624; Maillot, 285; Bonnet, 214; Bailly, 162, &c.

chiefly in some irregular mode of application of common atmospheric influences, which operate by occasioning, after a comparatively short, or even without any visible sedation, an inflammatory reaction in the organ affected, as also secondarily in the system at large. In the other case, the cause consists evidently of a peculiar poisonous matter floating in the atmosphere; the primary and prominent effects of which are subduction of the vital or nervous power, diminution of vascular action, prostration, to a greater or less extent, of all the energies of life, and, as a consequence, of the vital affinity and cohesion of the soft solids and contamination of the circulatory and secretory fluids. These are doubtless attended, in the early stage of the milder cases and less violent forms, with symptoms of reaction, as well as with a functional derangement of important organs; but often this reaction is broken or imperfect; and frequently the disease is characterized, throughout its whole progress by symptoms of prostration. In a word, we perceive effects which bear, in the more severe and malignant forms of the disease especially, a close analogy to those occasioned by other and more tangible toxical agents. Like some of these, as oxalic acid or nicotine—the malarial and several other zymotic poisons, sometimes suddenly prostrate the system to the verge of the grave, or even destroy life in a few hours and during the first access, or, as Dr. Simon says, in the tremendous shock and depression thereby occasioned in the system. So rapidly destructive, indeed, is the effect, that were it not for concomitant circumstances it would often be difficult to form an idea of the real nature of the case. The narcotico-irritating quality of the poison is fully indicated by the phenomena of the opening stage of fever, for this is marked by a reduction of vital energy, obtuseness of sensibility, suspended or perverted secretion, and diminished calorification. As Dr. Drake remarks: "We may assure ourselves that its first effects will not be increase, but depression of excitement, by referring to the constitutional influence of foreign matters, liquid or gaseous, when introduced into one of the serous membranes (as the peritoneum, for example) which are always those of depression as well as irritation."[1]

It may not be amiss to remark, while on this subject, that paroxysms of febrile excitement, assuming a periodic type, and which, though not identical with, bear some analogy to, various forms of

[1] *Op. cit.* 733.

malarial fevers, are not unfrequently produced by agencies of a debilitating character, even though the localities where they occur are free from periodic fever; and we know that in paludal or fever districts, attacks of the disease are sure to be brought on by the application of such agencies, and that experience has taught the necessity of avoiding, in such districts, a mode of living calculated to place the system below par. Of the first of these causes, one example will suffice. M. Renouard communicated to the Academy of Medicine of Paris, in 1847, an account of several cases in which periodic paroxysms of fever were produced in a non-malarial locality by copious losses of blood. In one of these instances the disease produced assumed a regular tertian type; in two others the fever was remittent. All were cured by a tonic and quinine treatment.[1]

The two diseases differ widely as regards the duration of the process of incubation.—Other reasons may be adduced in support of the opinion here advocated. In one of these diseases—periodic or autumnal fevers—the period of incubation or latency, *i. e.* the period during which the system tolerates the poison, and the latter remains apparently innocuous until reaction is brought about by some intrinsic circumstance of the depressing kind or otherwise—though considered as very short by Macculloch,[2] Nepple,[3] and a few others, is often found to be very greatly prolonged; while in every case of pneumonia the morbid effect soon follows the application of the cause. The yellow fever poison, though occasionally rapid in its effects, and attacking but a few hours or days after exposure, remains at times dormant in the system as long as fifteen, thirty, or fifty days. In one case mentioned by Blair (69, 70), the period seemed extended to four months. In a private communication to the author, Dr. Merrill states that during the epidemics of Natchez, which he witnessed, the period of incubation occasionally extended to fifteen days. M. Bertulus, who saw the disease in the West Indies, limits the period to from three to eight days, and affirms that it never extends beyond three weeks.[4] Dr. Stevens, also, who is well acquainted with the true yellow fever, which, in accordance with his views, respecting its origin and nature, he denominates African typhus, states that it never produces its effects previous to the fourth

[1] Bulletin, xii. 640. [2] An Essay on Marsh Fev. i. 20.
[3] Traité de la F. Int. 146.
[4] Mém. sur l'Intoxication Miasmatique, 39

day, and that he has known some well-marked cases in which, the poison being applied in a less concentrated form, the individuals were not attacked until twelve days had elapsed after exposure.[1] From all the facts I have myself been enabled to collect, I am of opinion that, though sometimes very short, and at others very long, the period of incubation in yellow fever usually varies from five to ten days.

In other forms of malarial fevers, the period of latency is seldom shorter than three or four days. In general it extends beyond this, and has not unfrequently been known to reach a limit far exceeding that noticed in the yellow fever. Our lake fever, according to Dr. Stevens, has an incubative period of about a week; but, when the poison is concentrated, an attack often comes on as early as the third day—never sooner.[2] In the epidemic of Naples, in 1764, the disease sometimes, though rarely, broke out immediately after exposure. In the greater number of instances, the poison remained latent to the end of the first week.[3] Dr. John Hunter states that, on the Watering service in the West Indies, some fell sick on the first and second day; others embarked and were seized on the tenth or fourteenth day, or even three weeks after exposure.[4] He informs us that the Suffolk militia were called, in 1793, from their healthy country to Hilson Barracks, the low, marshy, unhealthy situation of which is proverbial. Twenty-two died before they left at the end of June. In July the regiment, with eleven other battalions, encamped at Watertown, near Tunbridge Wells. One hundred sickened soon, out of five hundred, with fever. Some were taken ill in October, or four months after leaving the Hilson Barracks. The 18th regiment, in 1783, after being at the same barracks from June 22 to October 9, were sent to Gibraltar. There were then sixteen of the men labouring under ague. While at Gibraltar, though the regiment was only four hundred strong, the disease spread so rapidly among them, that by May the cases amounted to two hundred and eighty (including women and children), of whom a part were then recently attacked for the first time. Whilst such was the course of events in this ill-fated regiment, the disease did not exist in any other part of the garrison.[5] Dr. Hunter adds that, "ships returning from a warm climate, particularly if they have been in harbour during the

[1] Stevens on the Blood, 235.
[2] *Op. cit.* 243.
[3] Sarcone, Maladie de Naples, ii. 73, 74.
[4] *Op. cit.* 153.
[5] *Ibid.* 327–334.

unhealthy season, have many of their men taken ill of the remittent fever, even two or three months after being at sea.[1]

Some of the British soldiers who inhaled the pestiferous atmosphere of the Walcheren Marshes, were attacked for the first time in healthy situations in England—Colchester, Woodbridge, &c.—as late as nine months after they were brought back.[2] The following facts, communicated to Dr. Bancroft by Mr. Nixon, surgeon to a battalion of the 1st Regiment of Foot Guards, will be read with interest. The battalion landed on South Beveland on the 2d of August, 872 strong. On the 19th, the endemic appeared among the men, and between that day and the 4th of September, when the men embarked for England, *i. e.* sixteen days, 359 of them were attacked. The battalion was landed at Chatham about the 7th or 8th of September. Many of the men continued to be attacked with endemic fever, so that by the 8th of March, 1810, only 117 of the original strength had escaped the disease in question. Some of the 117 men were attacked with intermittent fever as late as the middle of the month of June.[3] Dr. Férrus, a distinguished physician of Paris, relates a striking instance of the kind. Three hundred men, of the old Imperial Guard, to which he was surgeon at the time, were exposed to the cause of autumnal fevers in Breslau. Many of them took the disease ten days after leaving the place; other cases followed, and the fever became general. Dr. Férrus himself was attacked six months after, while stationed on the Niemen, where no disease of the kind prevailed, and at a period when the country all around was perfectly healthy.[4] Of forty cases of intermittent fever which occurred on board the H. C. ship Barrosa, in 1832, 1833, three took place seventy to eighty days after leaving England; thirty-one while lying at Whampoa; and seven from two to three months after leaving Canton, and ninety-three days after the disease had ceased in the ship.[5] Labourers, especially the Irish, will go down, for harvest work, into Lincolnshire, and bring back the seeds of ague within them, and yet may not be attacked for weeks or months.[6] M. Boudin calls attention to a fact which he him-

[1] *Op. cit.* 335.

[2] Bancroft on Yellow Fever, 241, 304; Blane Diss. i. 244; Williams, Morbid Poisons, ii. 465.

[3] Bancroft, 307, 308, note.

[4] Diction. de Méd. xii. 6; *ibid.* 2d ed. xviii. 69.

[5] Peterson, Med. Gaz. xv. 269.

[6] Med. Gaz. xxviii. 365.

self noticed; that regiments that had returned to Marseilles, where periodic fevers are but little known, from the malarial districts of Algeria and Corsica, as well as those that had formed part of the expeditionary army of the Morea, continued during several years (*des années entières*) to suffer from diseases bearing unequivocal marks of the fevers of the localities they had left.[1] Towards the close of the year 1843, two regiments of infantry arrived at Courbevoie; the one (23d Light Infantry) coming from a northern garrison, the other from the citadel of Strasburg, where malarial fevers prevail extensively. The two regiments occupied the same barracks, performed the same duties, partook of the same fare, and were in every other respect on the same footing. Yet, while the first of these regiments suffered from typhoid fever and pneumonic inflammations, the other furnished, for more than a year, a large number of intermittent fever cases. The disease spread to several hundred of the men, and spared few—not even those who had not had it during their stay in the malarious locality whence they came.[2] A similar occurrence was observed in another regiment (the 75th) transferred from Strasburg to Versailles, in the autumn of 1843. In December of the year following, the fever was still prevailing, but only among the men who had arrived from Strasburg; the new recruits remaining free from the disease.[3] In another essay, M. Boudin remarks: "So far as regards myself, after a survey of the numerous observations we have collected in France, at periods and in localities exempt from periodic fevers among men arriving from the paludal districts of Corsica, Morea, and Africa, we have no hesitation in declaring that the period of latency of the malarial intoxication may extend to eighteen months."[4]

One of our countrymen, Dr. L. H. Anderson, of Alabama, in his Prize Essay, already referred to, states that he was himself attacked in the city of Paris, six months after leaving a mountainous district, with an intermittent of a very different character from the fever usually observed in that city. The remote cause, as he remarks, had no doubt been harboured in the system all the time, his general health appearing, notwithstanding, for three months before the attack, better than it had been for years. The exciting

[1] Annales d'Hygiène, xxxiii. 63.
[2] *Loc. cit.* 64.
[3] *Ibid.* 65.
[4] Boudin, Geographie Méd. 64.

cause, Dr. Anderson thinks, was doubtless the bad air of the hospitals, dissecting-rooms, &c.[1]

Dr. Lee relates the case of an officer of our navy, who was exposed several years before to the highly concentrated miasm which produced the fatal endemic among the residents of Thompson's Island, on the Florida coast. Although he escaped the bilious remittent, which proved so fatal to many others, he yet had occasional attacks of genuine intermittent, for several years afterward, on exposure to cold, moisture, or great fatigue, although residing in a part of New England where intermittents were never known to prevail.[2]

In all fenny countries, individuals exposed to the miasmata of autumn are often affected for the first time the following winter or spring; while exposure, at the latter seasons, in the same locality, is in no way attended with danger to those who have not imbibed the malaria. So frequently is this result obtained, that it is very generally admitted by the most accurate and cautious observers, that vernal or winter periodic fevers are the offspring of autumnal exposures. For the occurrenee of such cases in this country, we may, as Dr. Drake has done, refer to the experience of every physician who resides in regions infested with autumnal fever.[3]

This power of prolonged latency is an attribute of diseases arising from the agency of morbid and speeific, and of many common poisons—whether they are endowed or not with contagious properties—and its manifestation by autumnal fevers, establishes the fact of a close connection between malaria and the other class of morbid agencies alluded to, and indicates the propriety of referring that fever to a cause of kindred nature. According to Hildenbrand, the incubative period in typhus varies from three to seven days (p. 30). Haygarth[4] says, that of 72 persons exposed to the poison, 5 were attacked within ten days; 13 between the tenth and seventeenth day; 41 between the seventeenth and thirty-second; and 1 on the seventy-second. Of typhoid fever, we find that the period of latency, though usually short, is sometimes as long as it is in typhus, if not longer. Boudin states that at Algiers and Bone,

[1] Proceedings of the Ala. Med. Assoc. for 1852, p. 115.

[2] Copland's Dict. ii. 1090, note.

[3] Topog. &c. of the Miss. Valley, i. 813; Cooke, Med. Recorder, vii. 459; Edinb. J. lxxi. 355; Stevens on the Blood, 241.

[4] Letter to Dr. Percival, 68.

where this form of fever does not prevail or originate, troops arriving from Marseilles suffered from it for a few months, while others that arrived from elsewhere, or had been in the country some time, were exempt.[1] Dr. Williams states that the period may extend to five or six months.[2]

None of our readers need be told that some mineral poisons—as arsenic, mercury, and lead, for example—occasionally do not manifest their baneful or remedial effects until a long while after being absorbed; and may give, during a still longer period, proofs of their existence in the system, without, however, producing actual disease, or even indisposition. Orfila found, on the 3d and 7th of February, arsenic in the blood drawn from the arm of an individual who had swallowed a quantity of the metal on January 28th. In another case, he found the same substance in the blood twenty-two days after it had been taken.[3] The pestilential pustule (*bouton d'Alep*) does not break out, in some cases, until months after exposure. Boudin mentions a case in which it made its appearance in France ten months after the return of the individual from the East.[4] Aubert Roche states that, during a period of one hundred and twenty-two years (1717–1841), the incubation in plague was not found to be prolonged beyond eight days.[5]

The researches of the commission appointed by the Academy of Medicine of Paris, to investigate the subject of quarantine, lead to the conclusion that the incubation of the plague varies according to the period of the epidemic and other less influential circumstances. In the early period of an epidemic, the incubation is short. In the second period, and subsequently, its duration varies from three to five days. The larger number of the authorities consulted were of opinion that the incubative process did not exceed eight days; some thought it might run to the tenth day, or even beyond. Such cases, however, appear to have been rare. Nevertheless,[6] Pariset mentions a case in which the period extended to the thirtieth day.[7]

The usual length of the period of latency in hydrophobia is estimated at between thirty and forty days. Mr. Demeunynk, in a

[1] An. d'Hyg. xxxiii. 63; Géog. Méd. 50.
[2] Morbid Poisons, i. 40.
[3] Bulletin de l'Acad. de Méd. iii. 598, 676.
[4] Géog. Méd. 65.
[5] De la Peste ou Typhus D'Orient, 85.
[6] Rapport à l'Académie, &c. 196, 197; Clot-Bey, 19.
[7] Revue Médicale, Sept. 1844.

memoir presented to the Academy of Medieine, in May, 1839, relates three eases. In one, the period of ineubation extended to thirty-two days; in the seeond, it reaehed to fifty-four days; and in the third, the disease did not break out before three months.[1] Fothergill[2] mentions the ease of a Mr. Bellamy, who was bitten by a rabid cat, and was attaeked with that disease four months after. Similar eases are mentioned by Mosely and Dr. Matthei of Geneva; Vaughan extends the period to seven months; Fracastorius to eight;[3] Mead to eleven;[4] Bauhin to twenty-two; Dr. John Hunter,[5] R. Hamilton, and Nurse, to from seventeen to nineteen; Grant to twenty;[6] while Lentilius speaks of three years; M. Bouillod of seven years;[7] Dr. Bardsley of twelve years; and Morgagni of twenty, and even forty. Leaving out the latter very extraordinary eases, for the eorreetness of whieh I am not prepared to voueh, and whieh may have been, and probably were, the effeet of the imagination, or symptomatie of some other eomplaint, there is enough left to show that the period of lateney in hydrophobia is oeeasionally very long. The length of the period in eowpox varies from three days to three weeks. In a ease observed by M. De Lens, the disease did not show itself before a full month.[8] In another, mentioned by Dr. Stevens, the pustule did not make its appearanee until six weeks after vaeeination.[9]

The usual period in ehanere is from four to eight days, unless the surfaee is abraded. Hunter has known the disease to be retarded as late as seven weeks. Seeondary syphilis breaks out ordinarily between six weeks and six months after the eure of the primary symptoms; sometimes, however, not before several years. In one ease, mentioned by Williams, the disease did not show itself in fifteen years. The average ineubative period in smallpox is estimated at fourteen days, from exposure to the eontagion, to the appearanee of the eruption—the extremes being one and three weeks—five to twenty-three days, aeeording to Williams. In the greater number of eases, no uneasiness is experieneed before the eleventh or twelfth day after exposure. The period in searlatina

[1] Report by M. Dubois, Bulletin de l' Acad. iii. 929.
[2] Works, ii. 222.
[3] De Contag. 123.
[4] The Med. Works, 57.
[5] Cyclop. ii. 492.
[6] Med. and Phy. Tr. of Calcutta, ii. 51.
[7] Anglada, Traité de la Contagion, i. 269.
[8] Bousquet, Traité de la Vaccine. 525.
[9] On the Blood, 237.

is from a few hours to ten days. In measles, from ten to sixteen days.[1]

Process of latency governed by definite laws.—The results of observations made during a period of a third of a century, have inclined me to the opinion, which others had adopted long before, that the process of latency in autumnal fevers is ordinarily governed, as regards duration, by certain definite laws, analogous to those which preside over the progress, fluctuations, and return of the same and some other diseases. The late Dr. R. Jackson was of opinion that the aptitude to receive the morbific impression of the cause of fever, takes place more at particular periods than others; that it manifests itself more frequently about the fourteenth day after communication with an infected source, and that it is observed chiefly at septenary periods, the seventh, fourteenth, twenty-first, &c. from the time of exposure. This opinion was based upon the results of his own observations, "made upon numerous bodies of men; upon healthy men placed as attendants in infected hospitals, and upon healthy soldiers sent to concentrated sources of endemic fevers." Among such, fever scarcely ever appeared before the seventh day, commonly not before the fourteenth, and in numerous instances not till the expiration of six weeks, or even two months; though the cause of disease during the time was ordinarily in great activity.[2] Nor has it failed to be noticed that intermittent fevers exhibit, during their course, a tendency to a septenary revolution; that at those periods, either after the seventh, fourteenth, or twenty-first paroxysm, the disease has a disposition to terminate spontaneously. This was frequently verified in Florida, under Dr. Forry's observations,[3] and has been noticed sufficiently often elsewhere to justify its being viewed in the light of a well-established fact. In remittent fever, the same tendency is noticed, the disease having a particular disposition to a favourable critical change on the seventh, fourteenth, twenty-first, and twenty-eighth day.[4] Nay, more, there are not wanting facts to show that the tendency to relapse in autumnal fevers is governed by analogous laws. We know that in the form of fever, which, from its constant return at stated times, after apparent convalescence, has received the name of the relapsing fever, the symptoms,

[1] Williams on Morbid Poisons, i. 40, 120, 172, 214.

[2] Outlines, 247, 248.

[3] Climate of the United States, 283.

[4] Jackson, Sketch. i. 197; Copland, ii. 1101.

after the critical sweat of the fifth or seventh day, generally return on the fourteenth; and those relapses recur not only once, but several times. Other fevers, in like manner, show a disposition to recur at stated periods. In the government of Ufa (Russia), autumnal fever, which in that section of country is very common, attacks the patient every seventh day only, and is so severe that it generally proves fatal.[1] Similarly to what occurs in relation to the decline of the disease, or to its attack after exposure to the cause, the periods most remarkable, according to the ample experience of Dr. R. Jackson, for the recurrence of the symptoms are the septenary; the seventh, fourteenth, twenty-first, and twenty-eighth, with, as that high authority adds, new and full moon.[2] Whether the doctrine will bear the test of observation, particularly in its application to the period of latency, time must determine. Sufficient is it for our present purpose to call attention to the fact, that, should the statement of Dr. Jackson and others, relative to the period of attack after exposure, be well founded, it furnishes an illustration of the connection between the period of latency and the septenary revolutions of the system, so far, at least, as the number of those revolutions have been counted. To this I must add, that individuals who relapse with fevers, do so, in all probability, in consequence of the cause not being completely eliminated from their system, and remaining in a latent state during the period of exemption. If this be admitted, and we find that these relapses take place at regular septenary periods, we derive from that fact a proof that the process of latency is under the influence of the law of septenary revolution.

Much may be said concerning the above law relative to the period of seven days in fevers; for it is one which, as Dr. Laycock has forcibly shown, is of very general application in the explanation of vital phenomena in health and disease. Everything, indeed, leads to the conclusion that a period of seven days, and definite fractions or multiples of that period, are very prominent in the series of phenomena called critical days, and the operations of the system generally. It forms part of the great fact of vital periodicity, to which attention was called by some of the most ancient writers who have left records of their observations. The doctrine of septenaries

[1] Notices of Russia, United Service Journal, Jan. 1833, p. 49.
[2] Sketch, ii. 212; Outline, 304.

is literally as old as the hills. Originating probably with the Chaldeans or Egyptians, it formed part of the system of Pythagoras; and its application, to the phenomena of disease particularly, is discussed by Hippocrates, Diocles, Galen, Celsus, and others. In his ingenious speculations on the periodicity of the phenomena of life, Dr. Laycock has shown that the critical days of health and the critical days of fevers produced by the entrance of a poison into the system—whether that poison consist in malaria, or in a contagious matter—are identical; that the depression of the system will, in consequence, take place at those critical days, and that a fever depending on a poison is more likely to appear on those days than on any other. In confirmation of this, he states that the latent period in most diseases is regulated by weeks, as are also the latent period of animal poisons. He remarks that, in accordance with this law, the latent period of fever rarely extends beyond twenty-eight days. "If we take menstruation as a type of the critical days, and suppose that a movement takes place every seven days, gradually becoming more intense at each up to the fourth week, we have fever days, at least in every month in which the peculiar symptoms of the poison, whether malarious, exanthematous, or contagious, may exhibit themselves; probably the number may be greater; but if one or two of these days be passed over without an outburst of febrile action, it is scarcely possible that the third or fourth will."[1]

In the fevers of hot climates, which are all or for the most part malarial, the doctrine of critical days, such as it has been transmitted down to us from the days of Hippocrates, has been amply verified. On this subject, we have the testimony of high professional authorities.[2] In the fevers of Europe, it has been found equally applicable;[3] and in this country, when the efforts of nature are not too much interfered with by an *heroic* perturbative treatment, the Hippocratic doctrine often shines out in all its purity. To this fact, which some twenty or thirty years ago it would have been considered heresy to allude to approvingly, the eyes of our more

[1] London Lancet, Oct. 1842, p. 161.

[2] Poisonnier Fièvre de St. Domingo, 119; Desportes, Mal. de St. Domingue, i. 198, 232; Dazille, Maladies des Nègres, 36, 65; Bajon, Mem. sur Cayenne, i. 2d Mem.; Leblond, Fièvre Jaune, 43; Levacher, Guide Méd. 50.

[3] Hildenbrandt, Med. Pract. pt. i. chap. v.; De Haen, Pract. Med. pt. i. cap. iv.; Baglivi, Opera, 80.

enlightened physicians are opening. Even in sections of country where formerly the idea of critical days met with most opposition, a very different sentiment is now found to prevail in the minds of correct, careful, and enlightened observers. Let one example suffice. Dr. Anderson of Alabama, in a prize essay, already cited, after remarking that a second exacerbation takes place the next day after an attack of fever, generally after meridian, adds: "This, in turn, gives way during the night, or towards morning, and another remission, more or less decided, is observed. If the fever take the double tertian type, the next exacerbation will come on during the morning, and will be followed by another the succeeding evening. The fever thus continues, sometimes rising during the same hour every day, and sometimes later on alternate days, until the sixth, when, if the observations of the writer of this essay are worth anything, the fever (if it terminate favourably) has a decided spontaneous tendency to decline, and leaves the patient free from disease on the seventh day." Again: "The fever (malarial) is generally at its height on the fifth day; and, in bad cases, this is the day of danger. In malignant tertian intermittents, the fifth is the day for the third paroxysm, universally known to be the most hazardous; and, when death occurs in the disease, it commonly takes place either on that day or during the next." "Commencing practice," continues Dr. Anderson—whose fate, in that respect, has been similar to that of hundreds of physicians of this country—"with an utter disbelief in the doctrine of critical days, the contrary opinion has been forced upon me by actual observation. I am aware that the idea is discarded by a majority of physicians of the day; but I think that if any practitioner in the southwest will carefully note down the days in which malarious fever makes its appearance, and record accurately its subsequent course, he will find that the doctrine has a foundation in fact, and is worthy of some consideration. It is true, that the fever may often be made much lighter, or apparently arrested by the administration of quinia during the remissions; but it will generally be observed that the patient does not frankly recover, and that it is not until the seventh day that he seems actually well, and clear of all symptoms of the disease."[1]

But it is not necessary to enlarge farther on these topics. Enough

[1] Proceedings of the Med. Assoc. of Alabama for 1852, p. 107.

has been said to prove to the most fastidious inquirer that, in periodic fevers, the period of latency is occasionally remarkably long; that in these diseases, as in others appertaining to the class of zymotics, the incubative process, the occurrence of relapses, the duration of the attack, and the period of critical changes are under the control of certain definite laws. May it not be asked whether anything of the kind has been observed in regard to pneumonia and kindred inflammations? A prolonged incubation in such diseases may, for what I know, have been noticed elsewhere; but so far as the experience of the physicians of this section of the country extends, it may be safely averred that nothing of the sort has occurred, and I tax my memory in vain for a reference, bearing on this point, to some one of the many publications, foreign and native, that have passed through my hands. Never and nowhere have I seen, or heard, or read of a case in which the attack has come on, whether abruptly or preceded by premonitory symptoms, more than a few days after exposure to the cause. An incubation of eighteen months or a year, of a month, or of a fortnight, has never been, and probably never will, nor can be seen or heard of in such complaints. Neither can we find that the process of incubation or latency is under the controlling influence of any fixed law, and has a fixed and regular duration. The same may be said of relapses which may occur at any period during convalescence, whether on an odd or even day. Such, at least, may be presumed to be the case, for so far nothing satisfactory has been adduced to prove that the phlegmasiæ are, in regard to relapses, governed by the law of septenaries. How far the action of the same law extends relatively to the duration and periods of critical changes in those diseases, remains yet a mooted point. That the influence is exercised to a certain extent, may possibly be one day conclusively shown; for in some diseases other than those produced by malarial or contagious poisons—hemorrhages and nervous complaints—the intervals are often regulated by weeks; and in sundry acute and even chronic diseases, we not unfrequently observe—especially in the first, as Dr. Laycock well remarks—a movement of a tertian or quartan type, or in clinical phraseology, a good and a bad day.

In pneumonia, as in other affections attended with febrile reaction, the same tendency to a movement of the kind is often manifested, and the disease is found in many cases to end on particular days. But, so far as observations go, the exhibition of a tertian or

quartan movement, is not as generally noticed in such complaints as in the pyrexiæ, while the disease is far less under the control, in regard to the period of critical changes and to duration, of the law so often referred to. If exceptions are encountered; if cases of this and other kindred diseases are found, in which the critical movements referred to are as marked and regular as they are in autumnal fevers generally, it is principally among those on which the malarial poison has produced its impress, or which are combined with a malarial complaint. For this poison, while engrafting the periodic type on the diseases with which it combines, imparts to them many of the other peculiarities which characterize its legitimate offspring. True it is, that Dr. Traube has, in his experiments, found that in many cases of pneumonia the period of termination was marked by some critical discharge; and that in many the change began to take place—when the disease ended within the first fortnight—either on the third, the fifth, seventh, ninth, or eleventh day. Out of fifty-two cases of disease analyzed, the change suddenly took place in thirty. Out of twenty-seven of these, four ended on the third; nine on the fifth; eleven on the seventh; two on the ninth; and one on the eleventh day. In two of the remaining cases, the beginning of the disease could not be accurately fixed, and in one the change took place on the seventeenth day. Dr. Traube's reviewer, Dr. Herman Weber, remarks: "Amongst sixty-two acute cases, into which we lately accurately inquired with this purpose, in eighteen only could we ascertain the commencement of the disease; in five of these cases the notes on the temperature are not sufficient to be analyzed for the present subject; of the remaining twelve, the change took place rapidly in eight cases, and of these, on the fifth day, in two cases; between the fifth and sixth day, in one case; on the seventh day, in two cases; between the seventh and eighth, in one case; on the ninth day, in one case; between the ninth and the tenth, in one case."[1] But the number of cases in which the observation held good is, after all, but small; and of those in which it was verified, we are not positively informed how many were of pneumonia, and whether the disease was pure or complicated; and as we find that many of the individuals experimented upon were affected with typhoid fever, and as this, like other zymotic diseases, may reason-

[1] See Brit. and For. Med.-Chir. Rev. xi. 44, 45.

ably be supposed to be governed by different laws from common inflammation, we can derive no accurate information relative to the subject before us, so far as it applies to pneumonia, from the statements, interesting as they doubtless are, of those writers. Besides, if, in some cases of this disease, the influence of the law is found to be felt; in a greater number of others, not less accurately observed, it has failed to be so.

Andral remarks that pneumonia is one of those diseases in which the question seems the most easy to decide; because, on the one hand, the precise period of its onset is often very well marked; while, on the other, the period of its termination is often just as easily ascertained. One hundred cases gave the following results in the hands of this accurate observer:—

3	ended on the	4th day.
2	" "	5th "
6	" "	6th "
23	" "	7th "
2	" "	8th "
4	" "	9th "
11	" "	10th "
13	" "	11th "
1	" "	12th "
2	" "	13th "
11	" "	14th "
2	" "	15th "
2	" "	16th "
9	" "	20th "
1	" "	27th "
1	" "	42d "

Of the remainder, the period of termination could not be ascertained precisely.

3	lasted from	5 to 7 days.
12	" "	7 to 14 "
7	" "	14 to 20 "
4	" "	20 to 30 "

From this it follows that the days on which the larger number terminated, were the seventh, eleventh, fourteenth, and twentieth.[1]

[1] Clinique Médicale, i. 558, 559.

To a certain extent the result is favourable to the Hippocratic doctrine, though it must be borne in mind that the number of cases which ended on those days did not amount to more than one-half of the whole. On the other hand, the results obtained by Grisolle, are far from corroborating any conclusion favourable to the aforesaid doctrine that might be drawn from the facts heretofore adduced. This author studied all the phenomena of the disease, from the beginning to the end, in one hundred and thirty cases. In thirty-four of these, or about one-third, the period of the resolution coincided with some of the phenomena usually regarded as critical. In twenty-two, there was sweat, with or without deposit in the urine, either spontaneous or artificially obtained by nitric acid. In six, the disease ended by eruptions on the lips; in four, by urine; in two, by hemorrhage. To a certain extent the absence of critical movements in those cases may have been due to the kind of treatment pursued; and to the same circumstance may be ascribed the fact that these movements are less trenchant, or easily made out at present, than they probably were in Greece at the time of Hippocrates, whose treatment, as Baglivi[1] remarked long ago, was purely expectant, and did not disturb or oppose the efforts of nature. But to this cause alone we shall not feel disposed to ascribe the whole of the difference, if we bear in mind that, though more active now than it was in ancient times, the treatment, in the hands of judicious and skilful physicians, has for its main object the aiding nature, and that, if those critical movements were in pneumonia, as in fevers, the governing power, they would be promoted instead of impeded by the means employed. To this, let it be added that, in regions of country similar to Greece in respect to climate, cases of pneumonia in which the phenomena in question are well marked, the effect has usually been found to be the result of a complication with a malarial disease.

As to the period at which those critical phenomena manifested themselves, one of the cases observed by Grisolle ended on the fourth day; seven on the fifth; four on the sixth; three on the seventh; two on the eighth; eleven on the ninth; one on the tenth; and three on the thirtieth. Few, as will be perceived, presented salutary discharges on what Hippocrates regarded as the essentially critical days, the seventh, fourteenth, and twentieth. So far from

[1] Opera Omnia, 80.

it, the greater number of critical discharges (twenty) presented themselves on those days when the crises were considered by him as occurring less frequently, and as being less effectual. In eight, the change took place on empty days, *i. e.* those which were not considered as periods of crisis; and four presented critical movements on the sixth day, which, by the Hippocratic school, was viewed as the most pernicious, and hence was denominated, by Galen, the tyrant. From all this, Dr. Grisolle very justly infers that the doctrine of critical days is very unsettled, "and that it cannot be regarded as founded, so far as relates to pneumonia."[1] If, in addition to all this, we inquire as to the results observed in ten cases in which the disease was left to the powers of nature, we find that, in more than one-half of that number, the crisis took place on empty days; convalescence commenced in six on those days, and in four only on genuine critical days; a circumstance militating still more strongly against the doctrine in its application to the disease in question.

In some forms of autumnal fever the susceptibility of the system exhausted by one attack—not so in pneumonia.—It is a fact very generally conceded, that one attack of some forms of malarial fever exhausts the susceptibility of the system to the subsequent action of the poison giving rise to them. This is more particularly the case with respect to the yellow fever, which, according to the highest professional authorities in various parts of tropical climates, as well as in Europe and this country, seldom, if ever, attacks the same individual more than once. I am aware that exceptions on this point are to be met with; that by some they are represented as being quite numerous, and that by a different set of authorities they are maintained to be so frequently encountered as to invalidate or annul any rule attempted to be established on the subject.[2] But, notwithstanding all that has been said in favour of this opinion, the experience of those competent to decide in the matter, will justify the conclusion that instances of the repetition of the disease are comparatively rare; that one attack of true yellow fever serves, if

[1] Traité de la Pneumonie, 324, 325.

[2] R. Jackson, Yellow Fever of Spain, 50; Edinb. Journ. lxviii. 497; Bancroft's Sequel, 42; Rush, iii. 87, 88; Maclean on Epid. i. 233; Pinkard's Notes on W. I. ii. 257; Tulloch's Stat. of Brit. Army, 4; Labat, ii. 74, iv. 307; Arnold, 62; Fenner, N. O. Journ. v. 206.

not always, at least in the large majority of cases, as a protection against reinfection; that when in the West Indies, and other sections of hot latitudes, second attacks present themselves, they most usually do so in individuals whose systems, after having passed through the disease, have been modified or renovated by a long residence in some cold region—or by spending much time at sea—or in whom the original attack was mild; or again among those who, after having experienced an attack of the fever in its sporadic form, or during the course of a mild epidemic, become exposed to an extensive and malignant visitation of the same. Cases of the kind, whatever be the circumstances under which they may occur, are, I repeat, few in number, and scarcely more frequently encountered than are second attacks of smallpox, scarlet fever, and kindred diseases, the protective power of which is fully recognized.[1]

It is more than probable that those who contend for the frequent repetition of the yellow fever in the same individual, will be found principally among the advocates of the identity of that and other forms of autumnal fevers. If this be correct, the instances of second attacks recorded or referred to, may reasonably be supposed to have been cases not of true yellow fever, but of ordinary bilious remittent fever, and we shall read with less astonishment of the individual mentioned by Dr. Potter, of Baltimore, who had the disease as many as eight times. Dr. Potter, as also Dr. Rush, Dr. McLean, Dr. Pinkard, Dr. R. Jackson, Dr. Bancroft, and others in this country and abroad, who speak so confidently of such frequent repetitions of the disease as an every-day occurrence in yellow

[1] Lining, ii. 490; Dickson, Phil. Med. and Phys. Journ. iii. 273; *ib.* Essays, 352; Irving, 31; Simons, 21; Francis, N. Y. Journ. i. 299; Currie, 15; Hosack. i. 385; Townsend, 247; Archer, Med. Rec. v. 61; Kelly, Am. Journ. Oct. 1847; Barton, Rep. 21; Cartwright, Rec. vii. 15; Wood, i. 304; Arejula, 191; Fellowes, xxiii. 67; Berthe, 336; Gilpin, Med.-Chir. Tr. v. 318; Amiel, in Johnson, on Tr. Cl. 269: Pariset, 97; Wilson, 73, 74; Pym, 29; Appendix, 302; Rochoux, 38; Pugnet, 348, 349; Chisholm, ii. 233; Savarési, 256; H. McLean, 8, 187; Caillot, 249; Gilbert, 76; Dariste, 38, 112; J. Clark, 19; Blane, ii. 147, 148; Forry, 205; Williamson. i. 311; Strobell. 202; Dickinson, 40, 47, 68; Doughty, 183; Copland, Dict. ii. 951; Mitchell, 128; Chapman, Phil. Med. and Phys. Journ. ix. 130; Seamen, Med. Repos. i. 319; *ib.* Fever of 1795, in Webster's Coll. 40; Blair, 85, 86; Hume, 241; Klapp, Med. Rep. vi. 472; Veitch, 110; Monges, N. A. Journ. ii. 58; Ashbel Smith, Trans. of N. Y. Acad. of Med. i. 59; Treat. on Yellow Fever of Galveston, 60; Davy, Notes on Blair, 85; Lewis, Fever of Mobile, N. O. Journ. i. 418, ii. 43, iv. 162; McCraven, Top. and Dis. of Houston, Texas, Trans. of Am. Med. Assoc. v. 669; Jameson, Dublin Journ. of Med. Sci. (N. S.) xvi. 358.

fever regions, are all firm believers in the identity in question; while we shall look in vain, in the writings of those who entertain different pathological views, for the admission that such cases should be regarded otherwise than as exceptional. But even were we to admit that the disease, in all the instances of repeated attacks mentioned, whether the second or the eighth, was really the true genuine yellow fever, we should not be justified, from that circumstance, in denying the protective power of the disease; not only because such instances are, after all, comparatively rare, but because occurrences of an analogous kind are encountered in diseases which possess that power in a marked degree. As already mentioned, second attacks of smallpox, scarlet fever, and other kindred diseases, are not unfrequently encountered—nay, it is doubtful whether yellow fever repeats itself more frequently than either; and cases are on record in which they have outdone in point of repetition, anything that can be related of the yellow fever. Dr. Davy informs us, on the authority of a general officer (whose mother was the subject), of a case in which the smallpox was repeated eleven times in the same person.[1]

Other forms of malarial fever afford, if we may credit the statements of respectable authorities, protection against reinfection. In the West Indies, there are several forms of fever distinct from the true typhus icterodes, and which Dr. Copland has denominated bilio-inflammatory or ardent fever, and adynamic marsh fever, which act as seasoning fevers to Europeans who arrive in hot climates; the former appearing in robust plethoric persons, who have emigrated to the West Indies, intertropical Africa, &c., the other in those less robust, or who have not been attacked by ardent fever.[2] Dr. Stevens also recognizes the existence of two fevers independent of the true yellow fever, which he denominates African typhus, and, like Chisholm, Pym, and some others, regards as a native of Africa. One of the other two aforesaid fevers, he designates the climate fever. It corresponds to the bilio-inflammatory of Copland, and is a seasoning fever, and, as a general rule, is not taken twice, unless the individual so attacked has left the West Indies and returned again.[3] The Batavian and Edam fever, which, notwithstanding some points of similitude with the yellow fever of the West Indies, and the occasional occurrence in it of a few of the

[1] Notes on Blair, 86. [2] Dict. ii. 1104. [3] On the Blood, 194.

symptoms appertaining to the latter, cannot be considered as identical with it, is also evidently a seasoning fever.[1]

The pernicious fevers of Algeria, France, and other localities—the periodic and malarial nature of which cannot be disputed, have never, in the experience of Dr. Boudin, attacked the same individual a second time.[2] After observing that, with one exception, the exanthemata, which occur only once, have a quartan type, Dr. Lacock[3] says that "it is at least a curious coincidence, that a person who has had a quartan ague is not liable to a second attack." Sydenham many years ago made the remark, and stated as a fact worthy of observation, that if any person be seized with a quartan who has had it, though long since, it terminates spontaneously after a few fits, of whatever age or constitution he be.[4] After him, Van Swieten, as Dr. Lacock reminds us, expressly stated and insisted upon the same fact; and Dr. Wallis, in his note upon the above passage in Sydenham, confirms the statement, and remarks that "it is known from observation, in the marshes of Essex, the fens of Cambridgeshire, and other places where intermittents are endemial, that those who have laboured once for a series of time under this complaint, which will sometimes continue two or three years with short intervals, and escape the fatal consequences, will afterwards live totally free, or subject only to very slight attacks, which spontaneously disappear; though this is not always the case."[5] Sir Gilbert Blane states of the fever of Walcheren, that it is well ascertained that strangers, if they survive the first attack, become thereafter much less liable to the endemic intermittents.[6]

Some of the physicians whose views are now under examination, will not find fault with me for calling their attention to the fact that second attacks of typhoid fever,[7] a disease which doubtless arises, in some instances at least, from peculiar malarial sources, or at any rate is most certainly localized where these exist, and which not a few of them regard as nothing more than a peculiar form of autumnal or periodic fever, are seldom encountered. By some of them, too, Asiatic cholera is held in much the same light. Let them say how many cases of second attacks they have observed in that disease.

[1] Johnson, *op. cit.* 151.
[2] Géographie Médicale, 46.
[3] Lancet, 1842, i. 162.
[4] Sydenham, Wallis's edit. i. 82.
[5] *Ibid.* 82.
[6] Dissertations, i. 224.
[7] Bartlett on Fevers, 98.

Dr. Barker, in his report of the Cork Street Hospital, Dublin, states that he has for some time entertained the opinion, that sufferers from fever, attended with petechial eruption, if they are not altogether secured by it from a second attack, are not, at least, so liable to it as those who have had no fever of the ordinary kind. And, in continuation, he says: "Though I have frequently made the inquiry, I have not found a patient in whom this symptom was distinct, who had suffered from the same fever on any former occasion. The analogy which this bears to other fevers, more especially to that which appeared at Gibraltar, and also to some exanthematous diseases, lend support to the opinion of its rarely occurring more than once in life."[1]

But while the power of exhausting the susceptibility of the system to future attacks appertains to some forms of autumnal fevers, as also to several other diseases, which many of the advocates of the views under examination have no hesitation in regarding as mere modified forms of periodic fevers, it would be wrong to attribute the same privilege to all the other forms of these. Experience, indeed, teaches, that however true it may be that the severe and malignant bilious remittent of the West Indies assumes, at times, the character of a seasoning disease, and guards against the return of the same—so long, particularly, as the individual remains in the country—the rule is far from being general, and does not apply to the common forms of the disease; for it is a well-established fact, that in temperate climates an attack of remittent or intermittent fever not only fails to impart perfect immunity to the sufferer, but is regarded by accurate and experienced observers as affording no protection at all, and even as increasing the liability of the system to fresh invasions of the complaint.[2]

If now we inquire how matters stand, so far as concerns pneumonia, in reference to the exhausting power in question, we shall find, that while that disease, and indeed all those of the same class, approximate to some forms only of malarial fevers, it differs widely from several of the more important ones; for, like bilious remittents and intermittents, but unlike yellow and other fevers mentioned, one attack of pneumonia, nowhere and under no circumstances,

[1] Bracken's Rept. in Barker and Cheyne's Acc't of Fev. of Ireland, i. 241.

[2] Ashbel Smith, *loc. cit.* 58; Dickson's Essays, &c. 344, 345; do. Charleston J. vii. 810; Simons, 21; Wood, i. 304; Tullock, Statist. 46; Strobel, 203; Parry, Am. J., Jan. 1843.

whether in the north or the south, the east or the west; whether the case be mild or severe, simple or complicated, affords the most remote shadow of protection against the recurrence of the disease. So far from its doing so, everywhere we find that persons who have laboured under pneumonia, are by that very fact rendered more liable to the complaint than they were prior to the attack. Hence, second and third repetitions are very frequently observed, especially in the same lung. The disease has been noticed ten times in the same individual by Chomel; eleven times by Frank; sixteen times in eleven years by Andral; twenty-eight times by Rush; three, four, five, seven, and eight times by Grisolle;[1] and there is scarcely a practitioner in this or any other country, who has not witnessed instances of the kind.

It is not to be denied that, on this particular point, pneumonia, while differing greatly from some of the forms of autumnal fevers, allies itself to others which do not possess the power of exhausting the susceptibility of the system to future attacks; and the advocates of the close alliance and identity of the two diseases may appeal to this circumstance in proof of the correctness of their views. But on reflection, the reader will, unless I am greatly mistaken, join in the opinion that the similarity in question can afford but little if any support to the idea of pneumonia being only a peculiar form of autumnal fevers. For, if an argument is built upon the circumstance that neither pneumonia nor the forms of malarial fever in question afford protection against future attacks of the same, what shall we do with the fact that other forms of those fevers possess the privilege in question? The latter fact is not to be ignored; and if the advocates of the identity of pneumonia with periodic fever are permitted to adduce in support of their views the absence of the exhausting power in both the former, and some of the varieties of the latter, no reason can be assigned for refusing to the opponents of that hypothesis the liberty of pointing out, in proof of the soundness of their theory, the existence of that very power in yellow and some other fevers, the malarial origin of which is as evident as that of common bilious remittents and intermittents. In this way, we should have both parties in the controversy appealing to opposite peculiarities in different forms of the same class of diseases, to prove

[1] Cyclopedia of Pract. Med. iii. 406; Chomel, Dict. de Médécine, xvii. 214; Frank, Intern. Cl. Obs. Seléct. 96; Andral, Med. Cl. 192; Désoteux, Dict. des Sc. Méd. xliii. 376; Grisolle Tr. de la Pneumonie, 111.

or disprove the identity of these with pneumonia. So far from conceding to the advocates of the unity in question, the propriety of deriving an argument from the similarity or absence of the power of exhaustion alluded to in pneumonia, and in common periodic fevers, there can be no difficulty in perceiving that the fact of the poisons of yellow, and of some other kindred fevers, exhausting the susceptibility of the system, must lead to very contrary conclusions, and fortify us in the belief of the propriety of establishing a line of separation between pneumonia and malarial fevers generally. These fevers, as we have seen, are, like other zymotic diseases, the offsprings of particular poisonous agents introduced into the system, and therein producing special effects; and the only difference between them, so far as regards the exhausting power in question, depends on this, that in some forms the morbid poison in its action on the system destroys certain materials or principles, the existence of which is necessary to enable the latter to be morbidly affected by the impress of that poison; while, in the other case, the poison possesses no such destructive effects, and leaves the system susceptible of being again affected by it.

While bearing this in mind, the reader must not lose sight of the circumstance that this power of exhaustion, though possessed by many of the morbid poisons, is not a necessary and indispensable attribute of them all; for among them not a few are found, which, while affecting the system in a specific manner, and occasioning diseases of a highly and often fatal character, do not impart to those who recover the power of resisting their future morbid impress. In this category, we may place a number of the animal poisons and venoms. The absence of the same power of exhausting the susceptibility of the system to reinfection, cannot, therefore, be adduced in denial of the toxical nature of the cause of the particular forms of autumnal fever in which it is noticed, and as lending support to the belief in the close connection referred to between such fevers and pneumonic inflammations. If we refuse to admit the toxical nature of those fevers on the score of the absence in them of the power in question, and acquiesce for the same reason in the propriety of severing them from zymotic diseases with the view to class them with pneumonia, we cannot object to rejecting from the list of morbid poisons the animal venoms and poison above alluded to, and classing the diseases they produce among the phlegmasiæ of the chest. The conclusion in the one case, would be just as

reasonable as in the other; and I close the subject with the remark that, in all malarial or autumnal fevers, the introduction of a poison, whether endowed with that destructive agency or otherwise matters not, is necessary to occasion the disease—a circumstance very different from what occurs in pneumonia, the production of which is in no way connected with the existence of a poisonous agent; and if the latter disease assimilates itself with some forms of malarial fevers in this, that, like them, it leaves the system susceptible to farther attacks, it differs from them in not being the result of a morbid poison, as well as in its phenomena, anatomical characters, and other circumstances already dwelled upon; while those fevers, though not exhausting the susceptibility of the system, present a close analogy to those that possess the privilege of doing so, in regard to their toxical origin, their phenomena, their anatomical characters, and the laws by which they are governed. Viewed, therefore, as a class, malarial fevers—like many other zymotic diseases—may be considered, so far as concerns second attacks, as differing from pneumonia.

CHAPTER VI.

PNEUMONIA AND AUTUMNAL FEVERS COMPARED IN REFERENCE TO THE POWER OF ACCLIMATIZATION—AGES, SEXES, AND RACES OF THOSE AFFECTED: PREVALENCE OF THE TWO DISEASES AT THE SAME TIME, AND IN RAPID SUCCESSION, NO PROOF OF IDENTITY.

The power of acclimatization does not extend to pneumonia—Still more strikingly do autumnal fevers of various forms and grades differ from pneumonia, on the score of the protection afforded against attacks of those diseases, by the peculiar organic changes, resulting from long habituation to the sensible or insensible qualities of the atmosphere of particular regions or localities, or to the poisonous materials by which that atmosphere may be contaminated. That such a protection is thus obtained, to a greater or less extent, in regard to all malarial and some other forms of fever, no one who has examined the subject with attention will feel disposed to deny. By long habituation to infectious localities, and to the high temperature of hot regions, the system becomes acclimatized, and thereby acquires the power of tolerating perfectly and permanently the poison, or of eliminating it as soon as received, without succeeding reaction. The observation is of old standing. Pliny, nearly twenty centuries ago, called attention to the fact, "that they who are seasoned can live amid pestilential diseases," and the statement has been confirmed by all subsequent observations.

Equally well ascertained is it, that the danger of infection among strangers increases in proportion to the coldness of their native land, or of their accustomed place of residence;[1] that protection in its entire perfection is only enjoyed by the residents of towns or cities subject to the disease; and that the inhabitants of neighbour-

[1] Savarési, 260; Bally, 268, 384; Dariste, 210; Fontana, 170; Arnold, 26; N. Dickinson, 13; Blair, 59.

ing, but more elevated and salubrious portions of the country, though less prone to the disease than strangers from cold climates, are, nevertheless, liable to suffer when they venture in an infected place.[1] Not less certain is it, that the protective influence of acclimatization is lost by a prolonged residence in cold climates;[2] that the children of the natives of, and acclimatized to, tropical regions, do not enjoy the same advantages in regard to protection as their parents,[3] but acquire them rapidly as they advance in age; that the residents of some portions of tropical regions suffer to a certain extent from the disease, on removing to another and more insalubrious part of the same regions,[4] or even to one differing but little in point of salubrity. We find also that individuals acclimatized to a yellow fever locality sometimes lose, to some extent, the protection they had enjoyed, by a long exemption of that locality from local sources of infection, or its being favoured, during several successive summers, with a cooler and purer atmosphere than before;[5] and that the same results obtain when individuals change their residence to places where the effluvia evolved, though not more detrimental to health, are of a different nature from those to which they were accustomed.

So far as regards the yellow fever, every medical writer, from the days of Towne, Warren, and Pouppé Desportes, whose works contain the earliest professional records we possess relative to the fevers of tropical climates, has dwelled upon the protective effects enjoyed in hot regions by those who are acclimatized to sickly localities.[6] The decided advantages resulting therefrom is adverted to

[1] Dariste, 37, 38; J. Clarke, 22; Humboldt, 772, 773; Keraudren, 24; Imray, Edin. Journ. liii. 94; Ferguson, Med.-Chir. Trans. viii. 144; Bancroft, 268.

[2] Rochoux, 40, 41; Maher, 884; Bally, 332, 340; Arnold, 26; McArthur, in Johnson, 350; R. Jackson, Fevers of Jamaica, 250; H. McLean, 187; Pugnet, 345, 346; Frost, Med. Repos. xii. 224; Dariste, 37; Savarési, 256; Bancroft, 268; Frazer, Med.-Chir. Rev. xiii. 347; Rufz, Med. Exam. iii. 109; N. Dickinson, 12; Veicht, 111, 112; Hume, 267.

[3] Musgrave, Med.-Chir. Trans. ix. 106, 107; Rufz, Med. Exam. iii. 106.

[4] Humboldt, 761, 771, 772; J. Clark, 1, 2; Pugnet, 346.

[5] Chervin, Report on Rufz's Mem. 44.

[6] Williams, 51; N. Dickinson, 11, 66; Edin. Journ. xxxvii. 154; Henderson, 5; Hume, 237; Wright, Med. Facts, &c. viii. 8; Jackson, Fevers of Jamaica, 250; H. McLean, 187; Madrid, pt. I. 32; Savarési, 260; Bally, 268, 332; Dancer, Med. Assist. 82; Lefoulon, 20; Leblond, 18, 227, 245; Manson, 5; Rochoux, 31; Caillot, 13; Chevallier, 3; Pinkard, ii. 472, 1st ed.; Gilbert, 75, 76; John Hunter, 19; Arnold, 26; Bancroft, Seq. 41; McArthur, Dis. of Barbadoes, Med. Obs. vii. 326; Peixotto, N. Y.

by Labat,[1] Ulloa,[2] Griffith Hughes,[3] Moreau de St. Mery,[4] Herrera,[5] Oviedo,[6] and other early, and by all modern, travellers and historians; and among the public at large the reality of the privilege was early, and continues to this day to be, viewed as placed beyond the possibility of doubt.

The whole history of yellow fever, as we are told by one who has thoroughly investigated the subject, shows that its most susceptible subjects in tropical climates are those who have recently arrived within its sphere, particularly the inhabitants of northern climates, and that the predisposition to an attack increases with the degree of the northern latitude from which the stranger has arrived, and the shortness of the interval that has passed since he left the European for the equatorial region. In illustration of the comparative security of native inhabitants over new-comers, this author —Dr. Ferguson—adverts to the experience of the troops at Cape St. Nicholas Mole, St. Domingo, among whom, soon after disembarking, yellow fever broke out "at every station and in every place." During the earlier part of the residence of the British troops there, while all were deeply interested to stop the mortality, a census was taken of the inhabitants of the town, exclusive of the negro slaves and the white soldiers, when they were found to be very nearly equal in numbers. But by the time they had buried the original complement of 1,500 men, the inhabitants had not lost more than one in thirty of all ages.[7]

Dr. Burrell states, that "of thirty regiments that arrived in the Windward and Leeward Islands, between 1816 and 1848, ten were attacked with black vomit fever a very short time after landing; two within three months; eleven within twelve months; five within two years; and two within three years of their arrival. Of thirteen regiments, which landed in Jamaica between the years 1816 and 1834, four were attacked within six months; seven within twelve months; and two within eighteen months. From 1838 to

Journ. i. 417; J. Clark, 22; Dariste, 21; Evans, 276; Gillkrest, Cycl. ii. 279; Boyle, 150; Blair, 59; Doughty, 65; Trans. Am. Med. Assoc. v. 589.

[1] Nouveau Voyage en Amérique, ii. 264.

[2] Voy. Hist. de l'Amérique Méridionale, i. 42.

[3] Hist. of Barbadoes, 37.

[4] Loix et Constitutions de Saint Domingue, i. 375.

[5] Historia General do los echos de los Castellanos in las Islas, &c. de Mar Oceano, lib. iii. cap. 15, lib. x. cap. 4.

[6] La Historia General de las Indias, lib. v. cap. 11.

[7] Ferguson's Notes and Recollections, 150.

1848, seven regiments arrived in that island, but the emancipation of the negroes permitting the troops to be quartered in the mountains, a few cases only of black vomit fever appeared within that period, in two of them soon after landing."[1]

It is, however, proper to remark that, general as the rule undoubtedly is, it is not universal, but subject to occasional, and, according to some writers, more or less frequent exceptions.[2] Phenomena of like nature are observed in our southern cities—New Orleans, Charleston, Savannah, Mobile—where the fever is, as it were, endemic, and the heat considerable and of long continuance, and where the causes of insalubrity assume a character of permanency. There, as in tropical regions, the natives and long residents who have gone through the process of acclimatization remain, with few exceptions—principally in times of severe epidemics, when the cause is very powerful—free from the disease, which exercises its effects among strangers; so exclusively, indeed, as to have acquired in Charleston, and some other places, the name of the stranger's fever. There, as within the tropics, true and effective acclimatization is enjoyed only by the residents of localities liable to the disease, and not by those who reside in country districts, or even in the suburbs of infected cities. Like West Indians, the acclimatized inhabitants of our southern cities lose the protection they possessed by a prolonged residence in some northern place, or in a rural district of the same region; while their children, and those of the natives, are as prone to the disease as strangers themselves.[3]

[1] Second Report on Quarantine, 9, Lond. 1852.

[2] Griffith Hughes, 37; Chevalier, 6; Gillkrest, 279; Pugnet, 331, 346; Caillot, 142; Bally, 332; Savarési, 134; Rufz, Examiner, iii. 109; Chervin, Rept. on do. 32, 44; Musgrave, Med. Ch.-Tr. ix. 106; *Ib.* Med. Ch. R. and J. iv. 981; Imray, Ed. J. liii. 79; Stevens, 195, 201; Chisholm, ii. 234; Bancroft, Seq. Ferguson Med. Ch. Tr. viii. 139–150; Jackson, 11, 35; *Ib.* Outlines, 63; Lempriere, ii. 29; Hunter, 19; Hillary, 126; Catel, 6; Desportes, i. 192; Humboldt, 771, 772; Dancer, 82; Guyon, 28; Warren, 4; Furlong, 290; Barry in Boyle, 270.

[3] Gros. Rept. 7; Girardin, 49, 51; Thomas, 1st ed. 77; 2d ed. 22; Michaud, Voy. a l'ouest des monts Alleganys, 5; Townsend (for fever of St. Augustin), 361; Ticknor, N. A. J. iii. 218; Barton, Lecture on Acclimatement, 3; Essay, 21; Strobell, 139, 160; Bancroft, 188, 192; Dickson, Phil. Med. and Phys. J. iii. 257; Ramsay, Rev. of Improv. 39; *Ib.* Med. Repos. iv. 218; *Ib.* viii. 366–395; *Ib.* Hist. of S. Car. ii. 88; Drayton, Rev. of S. Car. 27, 28; Dickson, Bell's J. iv. 112; Simons, 13; Rept. on Fever of N. O. of 1819, pp. 35 and 43; *Ib.* on Fever of 1839, p. 324; *Ib.* Fever of 1820, p. 6; Monette, Western J. iv. 339; Seagrave, Med. and Phil. Reg. iii. 442; Waring, 59; Daniel, 63; Dupré, Hays's J. ii. 382 (N. S.); Harrison, N. O. J. 130; Dickson, Essays, 343; Lewis, Fev. of Mobile, N. O. J. i. 417; v. 43.

The same, though to a more limited extent, is found to be the case in various of our less southern cities—as Natchez, Vicksburg, Grand Gulf, and Norfolk, where the protection afforded by acclimatization is not sufficiently effective to limit the inroads of the fever to strangers. For there the natives and long resident, while less liable than the latter, are, nevertheless, far from being entirely exempt.[1] There also, as well as in the cities of our Middle States and in Europe, where nativity or long residence affords no protection, the inhabitant of more southern or tropical localities, in which the yellow fever prevails endemically, and assumes often the epidemic character, exposes himself with impunity to the causes of the disease;[2] and there also, as in tropical regions, among the unacclimatized, the disease is less apt to attack natives of southern than those of northern climates—less Spaniards, Portuguese, and French, than Russians, Germans, Swedes, &c.

Nor is it less true that acclimatization extends its influence, though less effectually and less generally, to some other forms of malarial fevers. In England, says Dr. Pinkard (ii. 480), "the harvest-men and strangers who go into the fens of Kent and Lincolnshire in the autumn, are more readily attacked with the endemical fever of those provinces than the inhabitants who constantly reside in the atmosphere which causes it." The protection afforded by acclimatization against endemic remittent is found to be enjoyed in this country, often giving entire immunity, and generally lessening the violence of the disease. Dr. Coventry mentions it in reference to our lake fever.[3] It is also recognized as regards the fever of our Southern States—Georgia and South Carolina.[4]

The term acclimatization, as we are told by Dr. Fenner, is just as familiar to the inhabitants of all the southern portion of the Mississippi Valley as it is to the citizens of New Orleans, "and is used to

[1] Merrill Chapman's J. ix. 246; Cartwright, Recorder, ix. 15; Taylor and Hansford, Med. Rep. iv. 206; Selden and Whitehead, *ib.* iv. 334; Archer, Recorder, v. 61.

[2] Bancroft, 266; Berthe, 167–169; Fellowes, 59; Gilpin, Med.-Ch. T. v. 317; Pym, 25; Arejula, 183, 330, 446; Pariset, 14, 15; Caisergues, 200, 201; Rochoux, 123; Shecut, 108; Seamen, Fev. of 1795 in N. Y. 7; A. Hosack, 10; Warren, in Tytler, 501; Potter, 162; Drysdale, Med. Mus. i. 39, 40; Dalmas, 100; Ramsay, Edin. J. viii. 429; Deveze, 55; Rush, iii. 80; Currie, 12; *Ib.* Barton's J. ii. 38; Cathrall, 6; Nassy, 38; Carey, 67; Condie and Folwell, v. vi.; Caldwell, Fever of 1805, 79; Facts and Obs. by College of Phys. 19; O'Halloran, 119; Bartlett, 461.

[3] Address, 42.

[4] Pendleton, Charleston J. vii. 451; Lewis, N. O. J. i. 323.

express the same idea, viz. that persons coming from a northern climate and settling there, are very liable to have attacks of fever during the first two or three years, but afterwards become quite exempt.[1] The same power of resistance to the action of disease by the acclimatized has long been recognized in other countries—in South America, in Italy, &c.[2] Lancisi says that they who are born and reared in the neighbourhood of marshes enjoy good health in an unhealthy atmosphere. And children can, by degrees, be accustomed to take considerable doses of the poison, beginning with small ones at first. "By this power of habit does it happen that man can live upon the small islands in those ponds and lakes; and that even the floating islands which the winds drive about from place to place, have their human inhabitants. For Secundus, in his description of the Lake of Vadimon, declares he has seen them. P. Cabæus has observed them in the marshes of Ferrara, and I myself have seen them in the Alban Lake."[3]

Dr. A. Brown, whose observations have reference evidently as much to the common remittent as to the yellow fever of tropical climates, after stating that a soldier, or stranger, cannot be considered inured to such climates by a residence of less duration than from three to five years, remarks that if the latter period be adopted, as affording a greater probability that a regiment has passed through at least one epidemic season, on turning to table 76, p. 92, of the Statistical Report, we find that the average mortality, during the five years of service, was in the ratio of 147.7 per 1,000, whereas, in the second five years, it was 104.7 per 1,000 of the strength. If we now examine table 77, p. 93, from which the great epidemic years are excluded, we find that the mortality in the first five years of service was 102 per 1,000, and in the second five years 82 per 1,000 of the strength. From this we perceive that the immunity, or advantage enjoyed by the acclimatized, is not confined to epidemic years, though in such it appears to be about twice as great as in ordinary years; the diminution of the mortality, amounting in the former to four, and in the latter to two per cent. of the force respectively.[4]

[1] Southern Med. Rep. i. 32.

[2] Weatherham, Celle Hyg. des Pays Chauds, 76; Macculloch on Remittent Fever, i. 10, 11; Johnson on Trop. Climates, 151.

[3] De Nox Pal. Effl. lib. i. cap. v. 21.

[4] Second Report on Quarantine, 294, 295.

The following remarks of Sir Gilbert Blane on this subject, though applying only to the Walcheren fever, may be appropriately introduced here: "One of the most important circumstances in the operations of marsh miasmata on the human body, is the power of habit in mitigating its influence. The natives are a robust people; they are of a very wan and sickly hue, with flaccid flesh, and have all suffered more or less from the bad air which they breathe. The children of both sexes are very subject to glandular and abdominal complaints; and the adults, particularly those of the lower orders, have all of them, sometime or other in the course of their lives, laboured under the endemic intermittent. They are, however, infinitely less subject to intermittent fevers than strangers." "These strangers are also variously affected, according to the district from which they come. It was found, that of the British troops, the natives of mountainous countries and dry soils, such as the Highlands of Scotland, were more frequently affected than the natives of flat and moist districts, such as Lincolnshire." Sir Gilbert mentions the case of a French regiment, "which suffered in the second year of its being there only one-half of the sickness and mortality which it suffered the first year, and hardly suffered at all the third."[1] Monfalcon informs us that the same fate attends the inhabitants of the high and healthy localities in the vicinity of the fenny districts of the Bresse, who visit the plains in quest of work during harvest;[2] and similar statements are made by Foderé in regard to the peasantry of Romagna, Modena, and Ferrara.[3]

From all the accounts that have been transmitted to us of the sickness and mortality of French troops in Algeria, as well as of the results—in a sanitary point of view—of the colonization of that country, we learn, that the Europeans, from the north particularly, experience great difficulty in becoming acclimatized there, and that their chances of success in that respect are, to say the least, extremely precarious.[4] Diseases—miasmal fevers particularly—carry off a large number of the troops; a larger number are soon invalided, and the rest must, sooner or later, be sent back to France to renovate their constitutions. While such is the result among the European troops, the natives of the country, and the

[1] Rept. on Mission to Walcheren, Dissertations, i. 223, 224, 225.

[2] Traité des Marais, 196.

[3] Med. Leg. v. 167.

[4] Perier, An. d'Hygiène, xxxiii. 307; Boudin, *ib.* xxxvi. 381; Maillot, Fièvres Int. 265; Haspel Mal. de l'Algerie, i. 78.

Arabs enrolled in the army, and doing duties similar to those assigned to the former, remain free from fever, or take it in its mildest forms. Similar results are obtained along the shores of the Red Sea; for while the natives are exempt from the fevers of the country, and individuals of the Indo-Ethiopian race easily become inured to the climate, the Europeans are with extreme difficulty acclimatized.[1]

Chomel and other writers state that typhoid fever attacks more readily those who have been only a short time in Paris, while it in a great measure spares the natives of that city. It appears from a table published by Dr. Davidson,[2] that among 568 eruptive cases of typhus, in whom this point was ascertained at Glasgow Fever Hospital from November 1, 1838, to November 1, 1839, 176 were natives of that city, and 392 were strangers; 206 of these strangers had resided in Glasgow only from one day to two years, and 186 from two to twenty years and upwards. Dr. D. draws the following deductions: 1. That strangers are more liable to become infected with typhoid fever than native residents. 2. That the majority of strangers are infected within a comparatively short period of their residence in Glasgow. 3. That a minor portion of strangers, like the natives of Glasgow, may escape infection for many years, and yet be afterwards attacked.[3]

In 1851, typhoid fever prevailed epidemically at Montpellier, especially among the troops of the garrison. It was principally rife among those who had not been long in the place. So far as civilians were concerned, it more generally attacked workmen going the rounds of France (faisant leur tour de France), and recently arrived, as also people from the country who had come to work at the crops and the vintage; in a word, the disease selected its victims among individuals unacclimatized to the place.[4]

Having established the fact of the immunity—complete or partial—obtained from the action of the causes of autumnal, yellow, and other fevers, by long residence in malarial and infected localities, I am prepared to ask what analogy there exists in that respect between those fevers and pneumonia. In regard to the latter, it is undoubtedly true, as we have seen, that it prevails more frequently

[1] A. Roche, An. d'Hyg. xxxiii. 36.
[2] Thackeray, Prize Essay on the Causes of Fever, 68.
[3] See also Bartlett, 102.
[4] Anglada, Traité de la Contagion, i. 124.

in some orders of climates than in others; but whatever be the extent of its prevalence, it is not less certain that, wheresoever it does show itself, no class of the population is exempt from its attacks. Acclimatization exercises no influence in that respect; natives and long residents are as subject to it—individual constitutions, temperaments, idiosyncrasies, habits of exposure to atmospheric vicissitudes, and other circumstances being the same—as strangers and new residents.

Pneumonia and autumnal fevers affect different races.—The two diseases do not differ less essentially in regard to the force with which they affect the several races. It can scarcely be necessary to remark, that in all countries subject to malarial fevers generally—whether the true and genuine yellow fever, or ordinary autumnal or periodic fever—the black race is to a greater or less extent exempt from the disease. So far as regards the former fever, the immunity enjoyed by negroes born and raised, or acclimated, in countries where the disease is endemic—the West India Islands and the coasts of Mexico, and Southern America—and especially by the natives of Africa, has been noticed and recorded by almost every writer. This immunity they possess in much greater perfection than the whites born, bred, or acclimatized in the same localities; and hence may be supposed to owe a large share of it to the peculiarity of their organization. Few among them take the fever; and those that do so, have it generally, though not universally, in the mildest form.[1] If, like the whites, they occasionally lose this protection by a prolonged residence in cold climates, and take the disease on their return within the tropics, the occurrence is less frequently noticed among them, and the attack is of a milder cha-

[1] Custin, in Duncan's Com. v. 149 (Am. ed.); Moseley, 146; Lemprière, ii. 29; Bancroft, 51, 270–272; Jackson, Fevers of Jamaica, 250; Hunter, 20, 308; Pugnet, 346, 347; Savarési, 256–258; Frost, Med. Repos. xii. 223; Rufz, Med. Examiner, iii. 129; *Ib.* Chervin's Rep. 30; Ferguson's Med.-Ch. Trans. viii. 121; *Ib.* Med.-Ch. Rev. Jan. 1840, p. 300; *Ib.* Recol. 142; H. McLean, 187; Humboldt, 772; Hume, 237, 238; Chisholm, i. 142, 225; Bourgeois, Maladies de St. Domingue in Voy. Interessants, &c. 417; J. Clarke, 3; Caillot, 14; Manson, 5; Bally, 269, 303–305; Arnold, 34; Dickinson, 12, 48; Evans, 276; McWilliam's Niger Exped. 128; *Ib.* Fev. of Boa Vista, 93, 94; Doughty, 50; Warren, 14; Wright, Med. Facts, &c. vii. 8; Henderson, 7; Bryson, 54; Pinkard, ii. 484; Copland, iii. 151; Leblond, 18, 245; McCabe's Rep. on Dis. of Warm Climates, 43; Diction de Med. xix. 151.

racter;[1] and if blacks removing there from northern climates suffer from the disease, sometimes even in its most aggravated forms,[2] the effect is far less frequently observed among them than among the whites.

In our Southern States, the blacks have always exhibited the same superiority in regard to immunity over the whites—subject, however, to the same modifications—not only in those sections of country or in cities where the protection afforded to the latter by acclimatization is very general, if not entire,[3] but in those places, as Natchez, Washington (Miss.), Norfolk, &c., where that protection is comparatively limited.[4] In our Middle States, the blacks have usually—though not always—escaped entirely, and among the few affected, the disease has almost invariably assumed a mild and tractable character. For proof of this, I confidently appeal to the records of the epidemics by which this city has been visited;[5] while, if we cross the Atlantic, and inquire how matters stand in that respect in the cities of Spain and in Leghorn, we shall find that there also the blacks have almost universally escaped.[6]

Nor is it less a fact placed beyond dispute, that blacks are much less liable than whites to other forms of miasmal fevers, and that, when attacked, they have the disease in a milder form. This comparative unsusceptibility was noticed in the West Indies from the period of the earliest introduction there of negroes. It did not escape the attention of Herrera (lib. 3 and 10), and other of the early historians, whose observations on the subject have been confirmed by every subsequent writer on the climate and diseases, not only of those Islands, but also of every other portion of tropical regions. "The negro," says Dr. Ferguson, "may also be said to

[1] Jackson, Dis. of Jamaica, 250; H. McLean, 187; Lemprière, ii. 29; Bancroft, 176; Savarési, *op. cit.*; Gillkrest, Cyclop. ii. 279.

[2] Frost, *op. cit.* 224, note; Ferguson, Bell's J. iii. 14.

[3] Lining, Essays and Obs. Phys. and Literary, ii. 409; Ramsay's Hist. S. C. ii. 85; Simons, 14; *Ib.* Address, 12; Daniel, 65; Thomas, 77; Chalmers, i. 39; Seagrave, Med. and Phil. Reg. iii. 420; Fever of N. O. in 1839, in Rev. Med. for 1840, 325; Ticknor, N. A. J. iii. 218; Dickson, 345; Lewis, N. O. J. i. 416; Bartlett, 345.

[4] Cartwright, Rec. 9; Selden and Whitehead, Med. Repos. iv. 335; Merrill, Chapman's J. ix. 246; Archer, Recorder, v. 61; Monette, Am. J. i. 245; Hogg, Western J. i. 413–416; Valentin, 90.

[5] Rush, iii. 81; Caldwell, Memoirs, 210; Cathrall, 6; Currie, 13, 14; Wood, i. 307; Deveze, 108, 109; Folwell, 57; Carey, 85.

[6] Berthe, 167; Caisergue, 191–200; Burnet, 242; Rept. of the Med. Acad. of Barcelona, 23; Pariset, Fièvre de Barcelone, 542.

be fever proof; and the marshy savannas, which lie low and scattered and unventilated, prove to him the most healthful abode. From peculiarity of idiosyncrasy, he appears to be proof against endemic fevers. To him marsh miasmata, which so infallibly destroy our white soldiers, are in fact no poison. The warm, moist, low, and leeward situations, where these pernicious exhalations are generated, prove to him congenial in every respect. He delights in them; for he there finds life and health, as much as his feelings are abhorrent to the currents of wind that sweep the mountain tops, where alone the whites find security against tropical diseases; but the black, when placed there, is almost infallibly struck with the bowel and heart complaints which prove so fatal to him."[1]

The comparative average prevalence of fever among the European and black troops of the British army in the West Indian and African commands, and the like mortality from the same disease, afford a striking illustration of the difference of susceptibility in question. Thus, we find that, during a period of twenty years, from 1817 to 1836, inclusive, the result was as follows:—

	Strength.	Fevers.	Deaths.
Jamaica Command.			
Whites	51,567	46,922	5,253
Blacks	5,729	634	47
Bahamas.			
Whites	535	506	85
Blacks	7,102	2,260	40
Honduras.			
Whites	320	221	27
Blacks	4,356	1,100	19
Windward and Leeward.			
Whites	86,661	62,163	3,195
Blacks	40,934	6,856	190
Mauritius.			
Whites	30,515	4,707	53
Blacks	1,395	121	
Sierra Leone.			
Whites	1,843	2,600	756
Blacks	7,581	405	18

[1] Ferguson's Recollections, Black Troops in the W. I. 207, 209. See also McCabe, *op. cit.* 43.

The following table will show the comparative ratio of mortality from the disease, per 1,000 of mean strength, in the various West India Islands, and other commands:—

Windward and Leeward Islands.	Whites.	Blacks.
English Guiana	52.2	8.5
Trinidad	61.6	3.2
Tobago	104.1	8.6
Grenada	26.3	4.8
St. Vincent	11.2	.9
Barbadoes	11.8	3.8
St. Lucia	63.1	5.2
Dominica	19.3	7.7
Antigua	14.9	1.7
St. Christopher	12.1	10.5
Average of whole command	36.9	4.6
Jamaica	101.9	8.2
Bahamas	159.1	5.6
Honduras	81.0	4.4
Sierra Leone	410.2	2.4
Mauritius	1.7	0.0
Ceylon	25.7	1.1

In all those localities, we find the average number of fever cases among white troops far exceeding that among the blacks, while the mildness of the disease among these is evinced by the smallness of the mortality they suffered, compared to that among their Caucasian comrades. In speaking of the sickness of the British troops in the Sierra Leone command, Major Tulloch says: "Fatal as the fevers of this colony have proved to the whole troops, the blacks have been but little affected by them; indeed, the attacks have been fewer, and the deaths have not materially exceeded the proportion among an equal number of white troops in the United Kingdom or other temperate climates. Though fevers are much more frequent and fatal among the whites than in the West Indies, the reverse is the case with the blacks."[1] By another writer, Dr. Bryson, we are told (p. 22), that the natives of Fernando Po are a healthy, athletic race of people, yet this island is more detrimental to health than

[1] Dis. and Mort. of the British Army (Western Africa), 16.

any spot in the known world; even the Africans from the continent are always sickly here. Of thirty white mechanics who arrived in November, 1827, all had the fever in a very short time; the number that died cannot be ascertained. A few were invalided, and five only remained in June, 1828 (pp. 22, 69, 70).[1] The reader is doubtless already conversant with the universal exemption of the Kroomen, not only from the common remittent of the coast, but from every deadly fever, as a circumstance rendering them of extreme utility to the coast squadrons, and to all traders.

The negroes of our Southern States are but little subject to malarial fevers; and, while on the rice plantations of Georgia and South Carolina, the white cannot reside for fear of the country fever, the negro works with impunity, exposed to the rays of the broiling sun, and to the humidity of the flooded fields.[2] Dr. Ketchum, in a report on the topography, sanitary condition, and vital statistics of Mobile, says of the slaves owned by the creole population: "They seem to be entirely exempt from the attacks of yellow and bilious fevers."[3] Another writer, Dr. Pendleton, of Georgia, remarks: "That the African is less susceptible to malarious influences than the white, I have believed from general observation heretofore. Although more exposed to the cold dews and hot sun of autumn, as well as having more filth about their habitations, they seem to be less liable to periodic fevers, and more readily recover than the white." In illustration of this, Dr. P. shows that, in the county he resides in, the number of idiopathic fevers among the whites greatly predominates over that among the blacks; being in the proportion of 14.5 to 10.4[4] After remarking that congestive fever prevailed epidemically in his neighbourhood, Dr. Lewis, of Mobile, says: "There were, in my professional circle, two blacks to one white; yet I did not see a single case of congestive fever in a negro, nor did I hear that any died of the disease in that section of country. I have made inquiries of several medical gentlemen who have long been practising in the country; their experience does not materially differ from mine; the fact is, that the remarkable exemption from yellow fever, which this race enjoys, extends in a great measure to

[1] See also Daniell's Topography of the Coast of Guinea, 134.
[2] Daniel, 64, 65; Wood, i. 240–267.
[3] Fenner's Southern Med. Rept. ii. 307.
[4] General Report on the Topography of Middle Georgia, Charleston Journ. vii. 455.

all the malarious fevers of hot climates; they may all have intermittent and light bilious fevers, as well as the milder grade of yellow fever; but it is only under extraordinary circumstances that these diseases affect them so seriously as to cause death."[1] Dr. Lewis remarks, that any one who weighs calmly all the influencing circumstances by which plantation negroes are beset, the vicissitudes of heat and cold to which they are hourly exposed, such as running from the fields during a shower of rain, sleeping in wet clothes, on a cold bluff, or earthen floor, from which they arise with a pain in the head, or cold stiffened limbs; their inattention as to the preparation of their food, which they eat in a crude, half-cooked state, it will be found that these, and not malaria, are the chief causes of the mixed undefinable fevers, dysenteries, and diarrhœas that annoy them.[2]

Now, if with all these facts before us as to the insusceptibility of the African race to malarial fevers, we inquire into the prevalence of pneumonic inflammation among them, we shall find but little calculated to justify the idea of the close alliance of the two diseases; for experience teaches, that the blacks are as subject as the whites to pneumonia, if, indeed, they are not more so.

Dr. Robert Jackson, whose authority no one will attempt to impugn, says of pneumonia: "It is the most common form of disease that occurs among the transplanted natives of Africa, whether such as are enrolled in the lists of the army, or such as are reserved for field labour; particularly in islands of a dry air and broken surface."[3]

In illustration of this, let the reader examine the results obtained among the African portion of the British army, in tropical climates, and compare them with those referred to relatively to fevers, and he will at once convince himself of the correctness of the statement.

[1] Lewis on the Yellow Fever of Mobile, N. O. Journ. i. 417; Duperier, *ib.* vii. 575: Grier, N. O. Journ. ix. 430.

[2] In a *relevé* of 1,036 cases of malarial fever occurring from 1843 to 1848, and furnished by Dr. Boling, in a report on the epidemics of Alabama, there were 671 whites and 365 blacks and mulattoes.—*Trans. Am. Med. Assoc.* v. 425.

[3] Sketch, ii. 83, 84.

	Aggregate strength.	Cases pneu. and pleu'y.	Deaths.	Ratio of cases in 1,000.
Jamaica command, 1817–36.				
Whites . . .	51,567	736	15	13.4
Blacks . . .	5,729	75	17	13.1
Bahamas.				
Whites . . .	535	3	2	5.6
Blacks . . .	7,102	176	15	24.6
Honduras.				
Whites . . .	320	2		6.24
Blacks . . .	4,356	43	3	9.7
Windward and Leeward.				
Whites . . .	86,661	1,975	113	20.5
Blacks . . .	40,934	1,823	160	44.5
Mauritius.				
Whites . . .	30,515	726	39	23·8
Blacks . . .	1,395	30	2	21·6
Sierra Leone.				
Whites . . .	1,843	15	1	8.14
Blacks . . .	7,581	81	12	10.69

The frequency of pneumonia and kindred complaints among the blacks of tropical climates, has, indeed, been noticed time after time, and recorded by the best authorities;[1] and has not less attracted attention in this country, whether the disease appear in its ordinary inflammatory form, or in that denominated pneumonia typhoides.[2] In middle Georgia, as shown by Dr. Pendleton, while the diseases of the respiratory organs prevail among the whites in the proportion of 13.8 per cent., the same diseases extend among blacks at the rate of 28.0 per cent.[3] Dr. Lewis, in like manner, represents the various forms of pneumonia as being particularly prevalent among the negroes of the South, especially in Middle and South Alabama.[4] "Pneumonia without subjective symptoms," says Dr. Cartwright, "is very common among them (the blacks). Intercurrent pneumonia is more common among them than any other class of people.

[1] Desportes, i. 5, 33, 92, ii. 273; Dazille, 113; Leblond on Fever, 77; Levacher, 39; Campet, 210, 211; Daniell, Topog. of Gulf of Guinea, 53, 94; Bajon, Malad. &c. de Cayenne, i. 73.

[2] Fenner's Southern Med. Repts. ii. 432; Charleston Med. Journ. vi. 838.

[3] Fenner's Repts. i. 335.

[4] Med. Hist. of Alabama, N. O. Journ. iv. 33.

It is met with in typhoid fevers, rheumatism, and hepatic derangements, to which they are very liable in the cold season."[1] According to Dr. Harris, of Alabama, negroes were attacked, during an epidemic of pneumonia which prevailed in Wetumpka, in 1851, in "somewhat greater proportion than the whites, and among the former the mortality was decidedly greater than among the latter."[2] Dr. Grier has also called attention to the same circumstance, and remarks, "that the negro is more liable to pneumonic attacks than the white race, and the complaint proves more fatal to them."[3]

Difference of susceptibility of the two sexes.—We are scarcely less justified in seeking for proof of dissimilarity between the two diseases in the difference of liability of each of the two sexes. As regards pneumonia, males may furnish generally a larger number of cases than females, and the disease in them may assume frequently its most severe character. But there are facts sufficient on record to warrant the conclusion, that this greater prevalence of the disease in the first-mentioned sex is not the result of an inherent susceptibility; but is due, when it occurs, to a series of fortuitous and modifying causes; more particularly to the circumstance that males—owing to the nature of their avocations and mode of life—are usually more exposed than females to the causes of pulmonary inflammation; and that in places where exposure is equal in both sexes, the disease manifests itself as frequently in the one as in the other. On this subject, statements, for which we are indebted to Grisolle,[4] Chomel,[5] Williams,[6] Valleix,[7] and others, can leave no doubt on the mind of the unbiased inquirer, so far as regards Europe. For, besides that in rural districts, where women are as much exposed as men, the disease does not manifest itself more frequently in one sex than in the other; in prisons, for example, where the material conditions of life are similar for all the inmates, the number of females attacked equals that of males. Nor should it be forgotten that among children, who are exposed to the same

[1] On the Philos. of the Negro Constitution, N. O. Journ. ix. 205.

[2] Rept. on the Epid. of Alabama, Trans. of Am. Med. Assoc. v. 373; see also Drake, N. O. Journ. i. 584.

[3] N. O. Journ. ix. 430.

[4] *Op. cit.* 114.

[5] Dict. de Méd. xxv. 161.

[6] P. 289.

[7] Guide du Méd. Prat. ii. 259.

influences, the disease has usually been found to bear with equal severity on the two sexes. Similar observations have been made in this country and elsewhere.

In regard to malarial fevers of various grades or varieties, from the simple intermittent to the deadly and malignant yellow fever, we arrive at different conclusions. In these, males, excepting in some epidemics mentioned by Musgrave (106), Catel (10), Rufz (32), and under circumstances of a special kind, are more frequently affected than females; and, as a general rule, it may be stated that when these are attacked, they have the disease in a milder form. That this comparative immunity on the part of females may, in some measure, be due to their more temperate habits, and to their being usually less exposed to the deleterious influence of night air, or, perhaps, as is presumed by Copland, to the state of the female constitution during the period of uterine activity, is doubtless true; but it is equally certain, that, after making every possible allowance for the efficiency of these causes of resistance, we still find enough to convince us that females are far less obnoxious to the impression of the febrile poison than individuals of the other sex. Speaking of paludal fevers generally, Dr. Williams remarks that in the West Indies, in civil life, a woman is esteemed twice as good a life as a man, and he adds that "in barracks the same difference of liability is observed between the sexes."[1] The same fact is pointed out by other writers in reference to all those fevers conjointly,[2] as well as by those who have described the ordinary forms of the disease—intermittents and remittents—in France, Africa, Germany, and Italy.[3] In 1,036 cases reported by Dr. Boling, of Alabama, there were 585 males and 451 females.[4]

In yellow fever, the difference of liability and intensity has been noticed both in intra and extra tropical climates. In the West

[1] Morbid Poisons, ii. 456.

[2] Pinkard, ii. 476; Hume, 237; Arnold, 34; Williams, 51; Henderson, 4; Madrid, pt. i. 32; Copland, iii. 139.

[3] Ramel de l'influence des marais sur la Santé de l'homme, Marseilles; Fodéré, on Epidemics; Hufeland, Journal der prakt, June, 1811; Eisenmann, Der Vegetation and Exlanger, 1835.

Ramel remarks: "Women are much less subject to the disorders of marshy localities. We have noticed this fact on the coasts of Africa, and several of our friends who resided at Cayenne, have assured us that in that colony more than one female has had six husbands."

[4] Tr. Am. Med. Assoc. v. 425.

Indies it was observed and is dwelled upon by Desportes,[1] Moseley,[2] Leblond,[3] Blane,[4] Pugnet,[5] Poissonière,[6] Bally,[7] Savarési,[8] Jackson,[9] Hunter,[10] Trotter,[11] Humboldt,[12] Caillot,[13] Dariste,[14] Dickinson.[15]

In this country, the same circumstances have been recorded by Deveze,[16] Rush,[17] Carey,[18] Barnwell,[19] Condie and Folwell,[20] Caldwell,[21] Valentin,[22] Drysdale,[23] A. Hosack,[24] Thomas,[25] Townsend,[26] Simons,[27] Waring,[28] Hogg,[29] Cartwright,[30] Perlee,[31] Merrill,[32] Archer,[33] Gros,[34] Rept. of N. O. Fever of 1819,[35] *ib.* of 1839,[36] Bartlett.[37]

In Europe, also, similar observations have been made by Arejula,[38] Berthe,[39] Fellowes,[40] Louis,[41] Bally,[42] Gillkrest,[43] Caisergue,[44] Palloni.[45]

I am not aware that the facts hitherto published, regarding the susceptibility of the sexes to typhoid fever, are as yet sufficiently numerous to warrant us in drawing any certain conclusions from them. In some of the hospitals and cities of Europe and this country, more females than males affected with this disease have doubtless been treated; but, neither from this circumstance, nor from anything as yet adduced, can we derive satisfactory evidence of the male sex being less liable than the female; for the reverse of

[1] Mal. de St. Domingue, i. 4, 195.
[2] Trop. Climates, 433.
[3] Fièvre Jaune, 95.
[4] Dis. of Seamen, 405.
[5] F. de Mauvais Caractère, 347.
[6] Mal. des Pays Chauds, 55.
[7] Typhus d'Amérique, 269, 299.
[8] Tr. de la F. J. 264.
[9] Fev. of Jamaica, 250.
[10] Dis. of Army in Jamaica, 201.
[11] Medica Nautica, i. 347.
[12] Nouvelle Espagne, 775.
[13] Fièvre Jaune, 15, 135.
[14] Fièvre Jaune, 218.
[15] Inflam. Endem. of W. I. 13, 82.
[16] Tr. de la F. J. 105.
[17] Works, iii. 80.
[18] Fev. of 1793, p. 74.
[19] Physical Investigations and Deductions from Med. and Surg. Facts, 374.
[20] Fev. of 1798, p. 5.
[21] Fev. of 1805, p. 78.
[22] Tr. de la F. J. 70.
[23] Med. Mus. i. 33.
[24] Diss. on Y. F. of N. York in 1795, p. 2.
[25] Tr. de la F. J. p. 73.
[26] Fev. of N. Y. in 1822, p. 253.
[27] Rept. to B. of H. of Charleston, 7, 14.
[28] Fever of Savannah, 60.
[29] Western J. i. 413.
[30] Med. Recorder, ix. 16.
[31] Philad. J. of Med. and Phys. Sc. i. 10.
[32] Phila. J. of Med. and Phy. Sc. ix. 246.
[33] Med. Recorder, v. 61.
[34] Rap. sur la F. J. de la N. O. in 1817, p. 7.
[35] *Ibid.* in 1819, p. 7.
[36] *Ibid.* in 1839, p. 324.
[37] On Fevers, 457.
[38] Breva Descripcion de la Fiebre Amarilla, 182, 438.
[39] Fièvre J. d'Andalousie, 354.
[40] On Pestilential Fever, 120.
[41] On Yellow Fever, 291.
[42] Typhus d'Amérique, 301.
[43] Cycl. of Pract. Med. ii. 279.
[44] Contagion de la F. J. 190.
[45] Osservazioni sulla Malattia, &c. 90.

the above result sometimes occurs. When it does not, the greater amount of females affected finds a ready explanation in the excess of female population in some of the cities where the observation was made, and the greater exposure of women to the infection in the capacity of nurses and attendants. But, however this may be, it has been proved satisfactorily by the statistics of every large hospital, and by the bills of mortality of some cities where a proper registration is kept, that as large a percentage of male as of female patients die of the disease.[1]

The following are the proportions which occurred in the epidemic at Gibraltar in 1828, among the civilians:—

Men	684
Women	286
Children	200

Difference of susceptibility of the two diseases at different periods of life.—Experience has shown that, with the exception of a few epidemics in which children appear to have suffered severely from autumnal and periodic fevers, these diseases are the attendants of adult age—the young and very old being comparatively little amenable to the influence of the poison, and, when attacked, having the disease in a milder form. Exceptions to this rule are to be found in those seasons when the infectious effluvia are concentrated, and unusually powerful; or when exposure to their influence is unusually great. As was mentioned in speaking of the comparative liability of the sexes, this ordinary exemption of the young and old may, in some measure, be accounted for by their less exposure to the action of the efficient cause. Nevertheless, a review of all the facts we possess on the subject, can leave no doubt on the mind as to a greater liability to infection in adults and individuals in the prime of life, than at any other period, as well as to the fact, that if during the course of some epidemics, or at the close of others, a larger proportion of children suffer, the result, on the one hand, must be ascribed to the fact that the adult portion of those exposed are acclimatized[2]—an advantage which children do not enjoy; and, on the other hand (as illustrated at Barcelona in 1821), to the cir-

[1] See Davidson on the Cause of Fever, 60, 61; Bartlett, 101.

[2] Catel, 10: Byam in Chisholm, i. 143; Arnold, 147; Statistics of British Army, 52; Ramsay, Hist. of S. C. ii. 85; Dickson, Phil. J. iii. 257; *Ib.* Eclectic J. iv. 112; Simons, 11.

cumstance, that the proportion of this class remaining unattacked or liable to be infected[1] was, owing to peculiar causes, greatly enlarged. Be this as it may, if we lay aside these apparently exceptional cases, and examine the accounts of those fevers as they prevail in tropical and extra-tropical regions generally, we shall find, that though children and old people are often attacked everywhere, they enjoy, to a very great extent, the exemption to which I have alluded. So it has ever proved in the West Indies, in Europe, and in this country, as we learn from numerous and reliable authorities.[2]

In a memoir on the influence of marshes on the duration of human life, Villermé, in drawing his conclusions from a very large number of observations collected in the paludal departments of France, states that the injurious influence is principally felt by young children. He adds: 1st. Infants under one year of age appear, relatively to their number, to suffer less than children from one to four. 2d. After the age of ten, the influence of marshes is less injuriously felt than before. 3d. It is, or appears to be, still less felt from fifteen and eighteen, to twenty-five; but, from thirty-five or forty to fifty or fifty-five, this injurious influence is more striking, though not as great as among young children. 4th. Finally, old people appear more able to resist the effects of the morbific influence in question, than individuals at any other periods of life.[3]

This, so far as concerns children, is somewhat at variance with what was said above; but it should be borne in mind that the amount of mortality among these is not to be understood as resulting from marsh fevers alone; but evidently includes that of children

[1] Copland, iii. 168.

[2] Chisholm, i. 141; Pugnet, 348; Savarési, 264, 265; Caillot, 15, 136; Moseley, 438; Bally, 269-296; H. McLean, 36; Manson, 6; Madrid, 32; Dariste, 218; Arnold, 34; Rochoux, 120; Pinkard, ii. 476; Hume, 238; Williams, 51; Dickinson, 13; Blair, 50; J. Clarke, 2; Arejula, 182; Sir James' Fellowes, 58, 420; Ch. Maclean, 15; Berthe, 170; Mem. of Acad. of Barcelona, 23, 44; Caisergue, 191: Louis, 260; Burnett, 493; Pariset, 454; Palloni, 9, 10; Cleghorn, 106, Rush's ed.; Deveze, 106; Caldwell, 78; Carey, 74; Condie and Folwell, 5; Monges, Matthieu, Rousseau (see Rept. of French Acad. 28); Valentin, 90; Thomas, 73-77; Harrison, 136; Barton, 20; Gros, 71; Cartwright, ix. 16; Merrill, 9, 246; Simons, 7, 8, 10; Townsend, 253; Shecut, 109; A. Hosack, 9; Waring, 60; Dickson, Eccl. J. iv. 112; N. O. Fever of 1820, p. 7; *ib.* 1839, p. 325; Archer, v. Rec. 61; Brown, 88; Bartlett, 344; Perry, N. O. J. April, 1844; Wharton, Am. J. July, 1843.

[3] Annales d'Hygiène. xi. 345, 346.

who die from all diseases. 2d. That in all situations, whether marshy or otherwise, the mortality between one and five, is much greater than at all other periods of life. If these circumstances are taken into consideration, it will be perceived that the conclusions arrived at by Villermé may be correct, without invalidating the statement made as to the greater susceptibility of adults to malarial fevers. It is to be remarked, also, that Villermé refers to the epidemic of Groningen in 1826. On this occasion, however, the mortality among children from one to five, though considerable, and large in proportion to that at some other periods of life, was smaller than among adults from forty to fifty, and from sixty to sixty-five. At Pantin, and other villages situate along the course of the canal of Ourcq, malarial fevers prevailed extensively in 1810, 1811, 1812, and 1813—at the period of the construction of that canal;—but children do not appear to have suffered more severely than others (352).

Typhoid fever may also be said to be a disease of adult age; for though children, and young ones too, as well as old people, are sometimes attacked, they are so comparatively seldom; the fever bearing with greater force on individuals from eighteen to thirty.[1]

When, with these facts before us, we inquire how matters stand in relation to pneumonia, we discover, contrary to the stated opinions of Hippocrates, and, long after him, of Morgagni, Cullen, and others, that, so far from sparing children, the disease is of very common occurrence among them.[2] The researches of our countryman, Dr. Gerhard,[3] of Dr. Rufz,[4] and other modern pathologists, have, it is true, led to the conclusion that primitive pneumonia seldom or never attacks children between the ages of two to five; but granting this to be confirmed by subsequent and equally accurate observation, which, seeing the result obtained by Barthez and Rilliet,[5] we cannot for the present be inclined to do, it is admitted that they are very liable to the secondary form of the disease. At every

[1] Chomel; Bartlett, 100; Cowan, Vital Statistics of Glasgow, 20; Geary, Dublin Journ. of Med. Sci. xii. 98, 99.

[2] Laennec, i. 547; Billard, Mal. des Nouv. nés, 1833; Valleix, ii. 256; Barthez and Rilliet, Essai, 77; Guersent, Dict. de Méd. viii. 76; Chomel, same work, xxv. 160; Williams's Cyclopedia, iii. 406; Grissolle, 97, 98; Swett, 79.

[3] Am. Journ. Aug. and Nov. 1834.

[4] Journal des Connaissances Méd. Ch. 1834, p. 101, &c.

[5] Maladies des Enfants, i. 108; *Ib.* Essai, 76, 77.

other period of infancy, the two forms, whether lobulated or diffused, are frequently observed; while, at all subsequent epochs of life, individuals are, in an almost equal degree, liable to both forms. I am aware, that by many the disease is supposed to bear with greater force on individuals between the ages of twenty and thirty, a result apparently confirmed by comparative tables drawn up in the hospitals of Paris, and more particularly by the following one of 630 cases collected by Grisolle:—

11 to 20 years	84
20 " 30 "	190
30 " 40 "	117
40 " 50 "	107
50 " 60 "	84
60 " 70 "	37
70 and above	11
	630[1]

But when it is borne in mind that the proportion of individuals between twenty and thirty is considerably larger than that of any other portion of the population of the place where the above results were obtained, and that persons of that age are usually more exposed than others to the exciting causes of the disease, we shall cease to be influenced in our conclusions respecting the periods of life most obnoxious to pneumonia, by the large number of cases there reported. Indeed, if we take this into account, we shall find that the excess of frequency at that age does not reach beyond one-tenth, and that the same may be said of the proportion between thirty and fifty. The number in old age, compared with the number of persons who have reached that period, is very large, and justifies the remark of Chomel, that pneumonia, far from being rare in old people, is the most frequent disease to which they are liable, and withal the most fatal.[2] Dr. Williams remarks that, from his own observations, he is inclined to consider young children as more frequently the subject of pneumonia than adults. Of fifty-five cases attended by Dr. Byam and himself at a dispensary

[1] Chomel, Dict. de Méd. xxv. 160; Grisolle, 101; Pelletan, Mem. de l'Acad. de Méd. viii. 335.

[2] *Op. cit.* 161; see also Grisolle, 101; Valleix, ii. 257; Prus, Mem. de l'Acad. viii. 13; Hourman and Dechambre, Archives, August, 1835.

in the parish of St. Marylebone, thirty-two cases were of the age of six years and under.[1] A still higher authority, Laennec,[2] is of opinion that the two extremes of life are the most exposed to the influence of the disease. Dr. Swett, a distinguished physician of this country, recognizes the greater frequency and fatality of pneumonia in infancy and old age;[3] and epidemic pneumonia has been found from the days of Sydenham to our own, to be rife and highly fatal among persons advanced in life.

Effects of the passions and emotions in the two diseases.—Nicholas Massa, very many years ago, said: "Multi ex solo timore et imaginatione inciderunt in febrem pestilentialem;" Pigray denominates panic "pabulum et nutrimentum pestis." Similar views have been entertained respecting the injurious effects of that and other depressing passions and emotions—anxiety, grief, and sorrow, as well of anger, and other exciting ones—as productive causes of Oriental plague, by Diemerbroeck, Chicoyneau, Rivirius, Hodges, Desgenettes, Larrey, Clot-Bey, and almost every writer on that disease. Equally explicit on the subject are Hoffman, Aretius, Cœlius Aurelianus, Fazio, Chirac, Senac, Falconer, Crogan, Home, Cullen, and Huxham, so far as regards pestilential, typhus, nervous, or petechial fevers. In our own days, Dr. R. Hamilton, of Edinburgh, has shown the influence of panic in propagating contagious fever, as exhibited in the Magdalen Asylum of that city, in the spring of 1821.[4]

Lind, who, like Vandermeer and others, had pointed out the baneful effects of mental depressions in the production of scurvy and other camp and ship diseases, and the benefit arising from cheerful and buoyant spirits, remarks, in respect to their influence in the production of autumnal fever, that it is quicker and more violent in hot and insalubrious situations, than in purer and cooler air. A fit of passion, he affirms, often brings on an instantaneous attack of fever; a violent fit of anger or grief will immediately produce a jaundice or the yellow fever; the sight of a corpse, or any object of horror, and even a shocking story told to a person, have been often known, through an impression of fear upon the mind, to bring on a delirium, sometimes a violent vomiting and purging, which vomit carried off the patient in twenty-four hours. In an-

[1] Cyclop. of Pract. Med. iii. 406.
[2] Traité de l'Auscultation, i. 547, 548.
[3] Lect. on Dis. of the Lungs, 79.
[4] Med.-Chirur. Trans. of Edinb. i. 296.

other work the same author reiterates the sentiment, remarking that it is a received opinion that *fear* is a cause of itself sufficient to produce, in certain dispositions, a bad or malignant fever; there being many instances in besieged towns, where no other reason could well be assigned for the rise of malignant disorders, than the dejection of spirits, grief, and panic of the inhabitants, occasioned by the bombardment, and the apprehensions of a violent death from some sudden assault of the enemy.[1]

I am not prepared to say from personal observation, nor have I now the leisure to examine, how far the opinion of Lind as to the great injury arising from the action of the passions, under the circumstances mentioned, exclusive of some other agency, may be founded. Especially am I not prepared to admit on so equivocal an authority as Lassis[2]—who, not content with discarding all idea of contagion, disbelieves the existence of infection also—that nine-tenths of the mortality attributed to yellow and other fevers, should be ascribed to the effects of the fear and panic under which every one labours to a greater or less extent in times of epidemics. But there can be no doubt that such affections of the mind, as also most others to which we are liable, will be found to play an important part in the production and aggravation of the diseases of hot climates, and of the summer season of temperate ones, whether in Hindostan, in Africa, in the West Indies, in this country, in Europe, or anywhere else. Upon this subject there can be no mistake. Facts and statements in relation to it will be found recorded in almost every publication extant on those diseases. In his account of the fever of Barbadoes, in 1817, Dr. Ralph mentions, in illustration of the influence of anxiety of mind in the development of the fever, the tendency it showed to affect different classes of persons. "First among the people of the huts it prevailed; then in the barracks. After we had lost one officer by fever, several others soon became affected; and, in like manner, when one of the hospital attendants had died, others soon fell sick, grew alarmed, and died." "Un caractere ferme," is, he remarks, the best preservative from fevers on all occasions. To this cause we may attribute, in some measure, the terrible mortality of the fever in Spain, "when fear,

[1] *Op. cit.* 149.

[2] Causes des Maladies Epidemiques, 88; *ib.* Calamités resultant du Système de la Contagion, 9, &c.

the handmaid of ignorance, reigns triumphant over the minds of the people."[1]

Similar statements are contained in the works of Warren, 20, 25; Desportes, i. 24, 96, ii. 264; Clarke, *Dis. of Long Voy.* i. 165; Hillary, 146; Hunter, 18; Lemprière, ii. 10, 11; Chisholm, ii. 53; Pugnet, 335; Amiel, *Johnson on Trop. Cl.* 270, 271; Chirac, *Mal. Pest.* i. 186; Berthe, 147; Pariset, 586; Rush, iii. 49; Currie, 10; Gros, 7, 8; Moultrie, 26; Deveze, 113; Johnson, *Trop. Cl.* 74; Maillot, 265; and of others, reference to some of whom will be found at the bottom of the page.[2] The same remarks are applicable to excess in eating, drinking, indulgence in the venereal act, exposure to the sun, &c., all of which are known, in times of epidemics, to be fruitful sources of sickness, and to bring on an attack in individuals who have been exposed to the action of malarial or other zymotic poisons. Were I, says Dr. Rush, to enter a city, and meet its inhabitants, under the first impressions of terror and distress from its appearance, my advice to them would be BEWARE, not of contagion, for the yellow fever of our country is not contagious, nor of putrid exhalations, where the duties of humanity and consanguinity require your attendance, but *beware of exciting causes*.[3]

While such is the case with malarial fevers of various grades and forms, it may be doubted whether anything of the kind can be pointed out in regard to pneumonia. There are no cases on record, so far as I have been able to ascertain, calculated to show that the disease has been brought on effectually by the depressing or exciting passions, as we have just seen in regard to fevers; and it may be presumed that the medical writer who should, nowadays, venture on the assertion, that a large number of patients had been seized with the disease in consequence of their fearing to be so, of their labouring under the depressing effect of panic, or of their experiencing a fit of anger; and that others, endowed with a greater

[1] Med.-Chir. Trans. of Edinburgh, ii. 23, 24.

[2] Caillot, 138; Savarési, 228; Gillespie, 132; Rochoux, 24, 114; Rouppe, 293, 296; Bally, 366; Catel, 17, 18; Rufz, 34; Osgood, 22; Dariste, iv. 23, 39, 63, 64; Lefort, 30, 31; R. Jackson, Outlines, 249; Gilbert, 71; Bancroft, 185; Ralph, ii. 63, 64, 77; Mouillé, 16; Repey, 16; Lorrein, 9; Mabit, 15; Hume, 236; Copland, ii. 10, 53; Blin, 21; Audouard, 435; McWilliams, 105; Archer, Med. Recorder, v. 66; Drysdale, Med. Mus. i. 34; Tooley, 16; Barton, 20; Perlee, Chapman's Journ. i. ii. 10; Barnwell, 373; Fever of Mobile in 1819, in Jenning's collection relative to the fever of Baltimore.

[3] On the means of preventing, &c. autumnal diseases, Works, iv. 126.

share of moral courage, equanimity of temper, or a fine flow of spirits, had escaped, would be likely to excite the surprise of his professional readers. Nor is it less evident, that, common as pneumonia is in certain regions and seasons, of all countries, our own included, the number of cases would be vastly increased, were indulgences in good eating and hard drinking, and especially in what good old Floyer quaintly denominated "womanizing," as certain to bring on an attack of that disease, as they are to occasion the development of fevers in persons exposed to a malarial atmosphere.

It is possible that as regards the importance attached by me to the preceding points, as also to many of those noticed on former occasions, I may err; and that for advocating them so earnestly, I may, in the opinion of some learned writers, exhibit myself in the light of one who has remained *sadly behind the times.* But, if I am really found amenable to so grave a charge, I have the consolation to know, that those who entertain contrary views, and may accuse me of ignorance, have not as yet produced anything on the subject calculated to establish the correctness of their own position, and likely to change the current of opinion on the question at issue among the enlightened and reflective portion of medical inquirers. Let the reader examine what has been written on the subject; let him inquire what are the sentiments entertained on these various points, in this country and Europe, and he will find in what I have said little more than the reassertion of opinions more than once advocated; and of facts and statements adduced by the very highest professional authorities. If I err, therefore, I do so in good company. And surely, under these circumstances, I can have no objection to being accused of upholding antiquated notions, of hazarding nothing but bare assumptions, and amusing myself with starting conjectures at variance with what is fancied to be the present state of knowledge; provided, not that my opponents should prove that they are right and I am wrong, for this might give them some trouble; but that the aforesaid authorities, to whom the medical world has heretofore found good reason to look up to as safe guides, should be regarded as open to the same sort of censure.

The prevalence of the two diseases at the same time and in rapid succession no proof of identity.—We have seen that those who advocate the idea of the unity of pneumonia with periodic fever, derive an argu-

ment in favour of their views from the circumstance that these diseases sometimes, or indeed, often prevail at the same time in the same place, or in neighbouring localities, or that they succeed to one another in the same situation, merging into each other as time progresses. If, as it is argued, intermittent, remittent, and other fevers, arising indubitably from the action of malarial exhalations, and if, as every pathologist acknowledges, these fevers are identical in nature —different forms of one and the same complaint—if they prevail together, or replace each other as the season advances; and, if from this circumstance of coexistence and succession we are justified in inferring the existence of a similarity of origin and of nature, so the coexistence of pneumonia with those fevers, and the succession of the former to the latter, must equally warrant the conclusion that they arise from the same cause, and constitute different forms of the same disease; "the difference in their characters—phenomenal and anatomical—arising in part from the season of the year in which they appear, the peculiar localities in which they occur, but mainly from the nature of the organs which become involved in the diseased action, the predominance of inflammation or congestion, and the character of their periodicity." Dr. Merrill, who seems to attach much importance to the circumstance of succession, and doubtless will not refuse to join in sentiment with those who appeal to that of coexistence, sums up, in a passage already cited, but which I must once more call attention to, the gradual changes which this supposed one and identical disease experiences as time advances. "In the spring, we are apt to find those diseases assuming names which have reference more particularly to this periodicity and general pathology. As summer comes on, the greater implication of the hepatic organs changes the name, or adds an epithet to designate a prominent symptom. In autumn, the chylopoietic viscera become more strikingly involved in the diseased action; and this, again, is indicated by an ever-changing nomenclature. But when winter approaches, and the subjects are exposed to sudden transitions of temperature, the thoracic viscera are called upon to bear the burden of local disease; and then it is that the names pleurisy, pneumonia, pneumonia typhoides, pneumonia biliosa, pleuro-pneumonia, bilious pleurisy, lung fever, &c. become familiar sounds."

The attempt made to prove the identity of the diseases in question on the ground of their coexistence and succession, differs in

nothing from that made to assimilate together, both as regards causation and nature, certain forms of febrile diseases, which, by other observers, are viewed as totally distinct from each other, and also by that which has not unfrequently been attempted, to deprive them all of an independent nosological position, and pathological individuality, and prove them to be nothing more than so many grades or forms of common autumnal malarial fever. But the argument founded on such occurrences is not more satisfactory in the one case than in the other. Were it valid, we should on the same ground be led to the conclusion that scarlatina, measles, hooping-cough, influenza, and the like, which sometimes coexist with or succeed to each other, are one and all offsprings of the same parent, and necessarily one and the same disease. We should be compelled to admit also that, inasmuch as measles or influenza are followed occasionally by angina, in some of its various forms, and often coexist with, and more frequently—though not universally, as maintained by Webster—precede autumnal or periodic fever, whether these present themselves in the endemic or epidemic garb, it necessarily follows that they must all be viewed as resulting from the same cause as such fevers, and as being mere modified forms of them—the difference depending, as in the case of pneumonia, upon the season of the year at which they make their appearance, the localities in which they may occur, or the organs and tissues which become involved, &c. We every day see other coexistences and successions of diseases which have nothing in common with each other in respect to pathology and etiology; and which are equally independent, on those points, of the ordinary autumnal or periodic fevers of the localities where they show themselves. Asiatic cholera, for example, must have preceded, succeeded to, and coexisted with, many a prevalence of such fevers, and in 1832 it existed simultaneously, in New Orleans, with the yellow fever; and yet no writer of note, save Dr. Merrill, Dr. Searle, and a few others, have ever seriously thought of holding up cholera as a mere form of autumnal fever—a disease which it no more resembles in respect to phenomenal and anatomical characters, mode of progression, and laws by which it is governed, than smallpox does scarlet fever. If it approximates it on any point, it is in appearing with greater force in localities noted for containing sources of malarial infection; a circumstance for which it has been supposed by some physicians of reputation in Europe, as Boudin, Maréchal, Mouchet, and others,

and a few—but very few—of equal stamp in this country, to belong to the *category* of malarial diseases. But on closer and more careful examination, we shall find reason to discard such notions, and to conclude that, in the case of cholera, as in that of other zymotic diseases which have nothing in common with autumnal fevers except their toxical origin, the effluvia arising from these foul localities, as well as putrescent food, impure water, &c., produce their deleterious effects, not by furnishing the efficient cause of the disease, but by predisposing the system of those exposed to their influence, to receive the morbid impress of that cause. Typhoid pneumonia, which some have, with equal impropriety, converted into a peculiar form of periodic fever, but which, from its symptoms, the season of the year at which it usually shows itself, and the places where it has at times reigned paramount, is evidently unconnected with this disease, has necessarily been found in some localities to succeed the latter, and may, so far as I now recollect, have prevailed simultaneously with it. Nevertheless, few pathologists will feel disposed, from this circumstance alone, to coincide with them in the views they have set forth as regards the nature of that complaint. All who have seen much of it, recognize it to proceed from a special epidemic cause, similar everywhere, and giving rise to phenomena which impart to it a character distinct from that of autumnal fevers. It requires no great stretch of diagnostic knowledge to perceive that it is nothing but a combination of pneumonia and typhus or typhoid fever. It reigned in this country from 1807 to 1820—principally from 1812 to 1814. During this period, typhus or typhoid fevers occurred extensively, while the pneumonia was superadded in some cases only.

Typhus, typhoid, and relapsing fevers have been found to prevail together in several cities and localities of Great Britain. In this country, typhus, and especially typhoid—the more common disease of the two on this side of the Atlantic—have not unfrequently coexisted, to a certain extent at least. More often they have succeeded to, or supplanted the common autumnal or periodic fever; and yet the soundest pathologists and most experienced and careful observers of the day—men whom the stiffest contemner of book authorities may not be ashamed to look up to with respect—have found ample cause to regard these two fevers, however true it may be they both belong to the class of zymotics, as distinct diseases; while the physician who, in this nineteenth century, should argue

from the fact of typhoid fever prevailing at the same time with, or succeeding to, or supplanting ordinary autumnal fevers; that it is merely a modified form of the latter disease, thus ignoring the fact that the former presents distinct phenomena, and gives rise to distinct anatomical characters; that it runs a different course; that it has a different duration, and appears often in places where malarial fevers have never shown themselves, or have ceased to appear, or at seasons of the year when malaria is not and cannot be evolved; that it is in all probability antagonistic of the other, and is propagated in a different way—would give no enviable measure of his professional sagacity and knowledge.

Admit, then, for a moment, the validity of an argument in support of the identity of pneumonia with autumnal fevers founded on the coexistence of the two diseases in the same locality, or the succession of the former to the latter, and the reverse, and we shall be forced, for the sake of consistency, to make other and kindred admissions, which, in the present state of knowledge, would appear rather awkward. That typhoid fever is not a mere form of common periodic fever, is a pathological fact, which, from what precedes, I take for granted every intelligent and well-informed physician knows full well. The typhoid is the predominant fever of a great portion of our Eastern States, and of many districts of France, and other localities where periodic fevers are not at all known or seldom encountered. Paris, London, and other cities, as we have seen, are not visited by intermittent and remittent fevers—while typhoid fever prevails there extensively. Now, if we regard the pneumonia, which in malarial districts occurs at the same time with or follows on the heels of, intermittent and remittent fever, as identical with them, on the ground of that coexistence or succession, there is no reason why we should not admit that cases of pneumonia, which are very common in New England, in Paris, London, and other places subject to typhoid fever, and which coexist with or follow that complaint, are due to the same cause that gave rise to the latter, and are mere modifications of it. The conclusion would not be more extraordinary in the latter than in the former case.

But there can be no necessity to enlarge on this matter. Already, in the opening chapter, attention was partially called to it, and enough was perhaps then said to justify the inference, that nothing favourable to the idea of the identity of pneumonia with periodic fevers could be made out of the fact that the two diseases coexist,

or that the former succeeds to the latter. It was there remarked that inflammation of the lungs prevails very extensively in places where remittent, intermittent, and other fevers of kindred nature are not observed; that it shows itself usually at seasons of the year when, if the ordinary causes of fever had at any time exercised their influence, they have been effectually or temporarily removed; that in places where periodic fevers prevail during a certain period of the year, they are put a stop to, in all their varieties, by frost. It was shown that while fevers are thus arrested, pneumonia, which had coexisted with them, instead of disappearing also, continues to prevail as it did before the accession of frost, or even is observed to spread more extensively; and that as the cause of the fever had thus been destroyed—as proved by the entire absence of its legitimate effects—the cases of pneumonia which continue to show themselves after the accession of frost, cannot be referred to the morbid agency of the cause in question, but are due to the operation of some other morbific influence, over which frost exercises no control, and differing consequently from the former. It was stated that the same causes which give rise to pneumonia *after* a stop has been put to periodic fevers by frost, must be similar to those that produce the disease *during* the prevalence of those fevers and anteriorly to the occurrence of frost. It was moreover argued that, if the cause which produces pneumonia *after* that event must, for reasons stated, differ essentially from that occasioning the fevers thus arrested in their course, the cause giving rise to the cases that appear *during* the fever season, must also be different from the febrile poison; that hence, when the two diseases show themselves together, two sets of causes are at work; that from this difference of cause we have reason to infer the existence of a difference in the nature of the diseases produced; and that when pneumonia, in the regular succession of the seasons follows on periodic fevers, after the accession of cold or frost, or at the period of atmospheric vicissitudes, it is not influenced in its production by the cause of those fevers. It appears, therefore, as the effect not of a gradual change from one form to another of the same complaint, but as the result of the creation of a different, or the continuance of an independent disease.

Pneumonia and autumnal fevers are not convertible diseases.—In order that coexistence and succession may furnish material towards

building an argument in favour of the identity of the two diseases, it is necessary that they should be combined with other concomitant circumstances. Separately considered, they can serve but little purpose, and lend but feeble aid in the settlement of the question.

The connection of intermittents with remittents, which has been appealed to, in illustration of the supposed identity under examination, affords but little support to the argument, because it is founded to a much greater extent on other and more weighty facts than mere coincidence and succession. Under much variety of aspect, as is remarked by a most eminent writer of this country, these fevers "possess many deep-seated analogies and identities; they frequently change from one type to the other. Thus, an intermittent turns into a remittent, and the latter, assuming the type of the former, is often seen to become, first a quotidian, then a tertian, and finally, a quartan. A simple intermittent may, in the third or fourth paroxysm, take on the character of a fatal congestion; and that which began with an aspect of malignity, sometimes emerges into simplicity and mildness; vernal agues attack those who, in autumn, had suffered under remittent fever, not less than those who had experienced the intermittent form; the sequelæ of all the varieties are almost identical; the same treatment, with certain modifications, is applicable to the whole." Surely, nothing of this kind is observed to occur in reference to many diseases, the identity of which is insisted upon on the ground that they coexist together, or follow each other in the same locality. The yellow and common autumnal fevers, though kindred zymotic diseases, and arising from malarial exhalations, are not, strictly speaking, convertible, though cases occur which exhibit symptoms characteristic of both, and the diseases may either blend together, or appear in rapid succession in the same subject. Remittents or intermittents never change into yellow fever, and *vice versâ;* their anatomical characters and sequelæ are not the same; those attacked with yellow fever in the autumn, are not affected with ague the following spring, and the aspect of the two diseases are not the same. In a word, they do not possess deep-seated analogies and identities similar to those existing between the various forms of ordinary paludal fevers. Still less allied to autumnal fevers in those respects, are typhoid or typhus fevers, and true oriental plague, which, under the fostering hands of some unitarian pathologists, have been admitted into the family of

[1] Drake, 704.

periodic marsh miasmatic fevers, there to keep company with yellow fever, Asiatic cholera, typhoid pneumonia, to say nothing of phrenitis, gastritis, gastro-enteritis, hepatitis, and, for what I know, peritonitis, cystitis, tonsillitis, nephritis, gout, rheumatism, and the rest of the forty or fifty different varieties of diseases into which, as we are told, the nosology of southern fevers might be arranged, and which constitute so many links in the chain of morbid action, extending from a septenary ague up to the most violent and fatal form of yellow fever. At a still greater distance, in these same respects, from autumnal fevers, do we find pneumonia. So far as I am aware, the physician is yet to be found who has discovered that pneumonia and periodic fevers are convertible diseases in the way that the several forms of these have been shown to be. A case of intermittent or remittent is not converted into pneumonia by injudicious treatment, or a case of pneumonia transformed into a mild intermittent by proper, or into malignant remittent by improper remedies; their sequelæ are not identical. Those who have pneumonia in the autumn or winter, do not run as much risk of suffering from vernal agues or summer remittents, as those who have passed through these complaints; they do not possess many deep-seated analogies and identities, and the same treatment would require more than trifling modifications to make it applicable to both.

In saying this much on the subject, I am far from denying the change from one disease to another. Such changes are of daily occurrence, and are observed in regard to almost every complaint to which the human system is subject. A case commences with symptoms of common remittent or intermittent fever, and at its close exhibits phenomena appertaining to yellow fever. In other instances the reverse occurs, cases of yellow fever ending with symptoms of periodic fever. Typhus, typhoid, or pestilential fevers terminate sometimes in the same way; while, at other times, cases which at their outset presented the characteristics of common intermittent or remittent fever, assume, as the disease progresses, those of the fevers mentioned. So also with regard to pneumonia and fever. Cases of the former not unfrequently, under peculiar endemic or epidemic influences, end with symptoms of autumnal, as also of yellow, or typhus, or typhoid fever. On the other hand, cases of periodic or other fevers sometimes terminate with symptoms of pneumonia.

Doubtless changes of the kind may, strictly speaking, be re-

garded as the effect of conversion; but the conversion herein noticed is not that of one form of a disease into another form of the same. It cannot be occasioned by an increased force in the cause of the disease first existing, by a difference in the state of predisposition of the person attacked, or by the peculiar mode of treatment pursued. It is not the result of a mere modification of one and the same thing; but a change of one thing to another of a more or less different kind; in a word, it is the substitution, partial or complete, of one disease for another. Such conversions of diseases are not of rare occurrence. They are, indeed, familiar to all practitioners. They often lead to evil or fatal consequences, or simply to the removal of disease without restoration to health; and, without feeling disposed to believe, with Sir George Gibbes, that one disease is always necessary to the cure of another, that just as many functions undergo a secondary derangement as are necessary for the cure of the primary one, and that no diseases occur but such as are curative in their effects or in their tendency,[1] we cannot doubt, taught as we are by daily experience, that the cure of one disease is often effected by the occurrence of another; but whether leading to baneful or salutary consequences, while many conversions are due to the transfer from one part to another of a specific cause, floating, as it were, in the system, as the gouty, rheumatic, &c., a large number are the effects, not of the operation of such a cause acting in this latter way, but, as already stated, of the slow or abrupt substitution of one disease for another; sometimes, though not necessarily, allied to it in a pathological or etiological point of view.

The late Dr. Hillier Parry,[2] of Bath; Dr. Mackenzie,[3] and others, relate interesting cases of the kind, and several have fallen under my own observation, embracing a variety of dissimilar complaints; and if such occurrences are admitted to take place in the cases mentioned, we can have no reason to doubt that the same will hold good in reference to those instances in which symptoms of one fever replace those of another; as also in those in which the phenomena of autumnal fever succeed to those of pneumonia, and *vice versâ*.

[1] Brit. and For. Med. Rev. xxiii. 591.

[2] Elements of Pathology and Therapeutics, i. 48, 306–320.

[3] Sketch of the Natural Cure of Diseases, Brit. and For. Med. Rev. xxiii. 590.

CHAPTER VII.

PNEUMONIA AND AUTUMNAL FEVERS, ALTHOUGH INDEPENDENT OF EACH OTHER, AS REGARDS NATURE AND CAUSE, COMBINE TOGETHER, AND FORM, LIKE OTHER COMPLAINTS, HYBRID DISEASES, WHICH MUST NOT BE CONSIDERED AS PECULIAR FORMS OF EITHER.

Cases of pneumonia marked by symptoms appertaining to autumnal fever, are the results of complications.—I am prepared to be told that, so far, my remarks can have had reference only to pure and idiopathic pneumonia, such as the disease is said to show itself in high and healthy localities and northern latitudes, and that they do not apply to that form of it which physicians in the south or southwest are in the habit of observing. I may be told, that cases there present themselves, in which the ordinary symptoms of pneumonia are more or less modified, and are associated, to a greater or less extent, with other phenomena which approximate the disease to autumnal fevers; and that while in other regions thoracic inflammation may be, for what we know—for on that matter doubts are occasionally expressed—independent of periodic fevers both in regard to causation and nature, the cases which prevail in malarial countries, and exhibit the phenomena in question, may be admitted to give countenance to the hypothesis under examination. Of the occurrence of such cases, no experienced or well-read physician can be ignorant. He must be fully aware, that in miasmatic regions they are frequently encountered, and that there pneumonia—like, indeed, very many other diseases—often, if not generally assumes, to a greater or less extent, the periodic type. So frequently are such cases observed in those regions, and so decided often is the tendency to this type, that it cannot be a matter of astonishment that some physicians, noticing a succession of instances of the kind, and yielding too readily to a spirit of exaggeration and hasty generalization,

pardonable in individuals of limited professional acquirements, but which ought not to be encountered in other quarters, should have been deceived, and arrived at the conclusion that, with few exceptional instances, in which they admit, on the authority of others, the disease to be idiopathic, "the pneumonia which prevails in this country, sometimes sporadically, and frequently as an epidemic, is really and substantially nothing more than a peculiar form of remittent and intermittent fever."

But when we view these facts more attentively, without being swayed by a favourite hypothesis, and with a full knowledge of the occurrence of kindred modifications in a number of other diseases, and of the diversified effects produced in many complaints by the various modifying influences that surround us, we shall not be long in perceiving, that the facts in question admit of an easy explanation, on principles very different from those contended for by the advocates of the identity in question, and in a way which renders unnecessary the disunion of the beforementioned class of pneumonias from the ordinary form of that disease. It is a fact well ascertained, and perfectly familiar to those who have investigated the subject of the progress and succession of epidemic or endemic diseases, that the type of the fever which prevails immediately before the outbreak of pneumonia—in other words, before the period of the year at which the usual causes of the latter are mostly encountered and operate with more force and effect on constitutions predisposed to their action arrives—impresses its own character on pulmonary inflammations. Hence at that season, low surfaces, the vicinity of mill-ponds, of the banks of streams, and of other localities, which before were the abodes of pure remittent and intermittent fevers, become the seat of pneumonias, which often assume a marked remittent, and not unfrequently an intermittent character—the result of anterior influences; while the same disease in other situations where malaria is not evolved, or at a more advanced period of the year, when it has been completely destroyed, presents nothing of the kind for a longer period.[1] The same combination of phenomena is necessarily observed for a longer period whenever pneumonia shows itself in localities where malaria continues to be evolved all the year round, or where, from the absence of frost, it is only moderated, and not completely destroyed. In many such cases the inflammation of

[1] Lewis, Med. Topog. of Alabama, N. O. J. iv. 28.

the lungs presents itself in combination with symptoms appertaining to ordinary bilious remittent fever, or hepatic or bilious derangement, giving rise to what is denominated bilious pleurisy, a form of disease accurately described by many American and European writers.[1] In other instances, the bilious symptoms are not so prominent, and the affection of the lungs is associated with those of simple remittent or intermittent fevers.[2] Indeed, instances of remittent and even intermittent pneumonia, pleurisy and pulmonary catarrh, in which the inflammation is complicated with symptoms indicating the existence in greater or less purity of the element of periodicity, are to be found described in the writings of the most reliable authors. Morton, who early called attention to them, and indicated the treatment they required, had seen a hundred such cases; and since his day they have continued to be adduced as objects of familiar professional observation, not only in Europe, but in this country also.[3]

The pleurisies which prevailed in Minorca, in 1745–46, and of which Cleghorn has left us so graphic a description, commonly began like an ague fit, with shivering and shaking, flying pains all over the body, bilious vomiting and purging, which were soon

[1] Bianchi, Hist. Hepat. i. 236; Stoll, Med. Constitution of years 1776–77, i. 50; Frank, pt. 2, ii. 350; Forsyth, Med. Rep. xii. 353; Williams, Med. Reg. iii. 454; Potter, Med. Recorder, iv. 404; Drake, i. 749, 765; Wood, ii. 38; Pelletan, Mem. Statist. sur la Pleuro-pn. Mem. de l'Acad. de Med. viii. 443; see Bulletin de l'Acad. i. 835; iv. 447; Eberle, Pract. of Med. i. 284.

[2] Matheson, Charleston J. iii. 152; Oliver, N. O. J. vii. 387–89; Day, *ibid.* ii. 578–587.

[3] Morton, Opera Omnia Hist. &c. 21; Brera, Journal de Sedillot, xxxiii.; Bailly, Fièvres Intermittentes, 253; Gouzée (of Antwerp), Archives, 2d series, iv. 71; Daniel (of Cette), Ephemerides de Montpellier, iv. 339, 357, 454; Gouraud, Etudes sur les Fièvres Interm. 103; Sarcone, Mal. de Naples, i. 202; Frank, ii. 352; Mouton, An. Clin. de Montpellier, xxvii. 133; Laennec, i. 391, 4th ed.; Alibert, 57; Bonnet, F. Interm. 76, 118; Detournel, Arch. Gén. April 1829, p. 255; Nepple, F. Int. 99, 124, 266; Roche et Berquin, Nouv. Elem. i. 440; Leonard and Foley, Researches sur l'Etat du Sang, &c. en Algerie, Mém. de Méd. and de Chir. Militaire, lx. 135, 209; De Renzi, Miasmi Paludosi, 121; Lauter, Hist. Medica Bienn. Marb. Rural, 5th and 7th cases; Strack, Obs. de Feb. Intermit., obs. 32, 58, p. 74; Sauvages, Nosol. Med. ii.; Abloing, Journal General de Medicine, lviii.; Matthei, Journal de la Société Med.-Chir. de Parme. ii.; Bonaldi, *ib.*; Archier, Journal de Med. 1784, lxi.; Evans, 54, 17, 71, 78; Chauffard, Traité des Fièvres, 254, 257; Macculloch, 385; Mongellaz, 221, &c.; Bibliothique Med. Jan. 1819, from Tr. of Med. Soc. of Lyons, N. A. Med. and Surg. J. viii. 193; Du Pré, Charleston J. v. 607, 608; Maillot, 17, 41, 127; Raymond, Mém. de la Soc. Roy. de Méd. iv. 72; Foster, Stethoscope, iii. 91–93; Blake, N. O. J. vii. 510, 511; Rochoux, Bulletin de l'Acad. i. 926; Cooke, Recorder, vii. 459; Meli, Sullo Febbre Biliosi, 54, 55.

succeeded by quick breathing, immoderate thirst, inward heat, headache, and other feverish symptoms. In a few hours, the respiration became more difficult and laborious; the most part of the sick being seized with stitches in their sides. In a few instances these complaints preceded the fever; in others they did not come on till the day after. Many were drowsy, and inclinable to sleep, but they raved at intervals, or were much disturbed with extravagant dreams. In the mean time, the internal heat was in several very moderate; in some less than natural; but, for the most part, it was so intense as to raise the thermometer to the 102d degree; and often in the afternoon to the 104th. The pulse was likewise very variable, not only in different persons, but in the same at different times. In some cases it was like that of a man in health, or even slower than natural, while the patient was in the greatest danger; so that it could neither be depended upon as a prognostic sign, nor as an indication of cure. Nor was the colour or consistence of the blood more to be trusted; in many it had a white or pale yellow crust, the serum being of the same complexion; but for the most part it was red and florid. Besides some abatement of the fever, which commonly happened every morning, it was remarkable that upon the third day, or beginning of the fourth, there was frequently a great remission, sometimes a total cessation of every violent symptom, so that the sick were thought to be out of danger; but on the fourth or fifth, a delirium suddenly came on, or the breathing became more difficult than ever, and one or both of these symptoms increasing hourly, the patient expired in a day or two, either suffocated or raving mad, unless, nature or art assisting, he had the good fortune to escape by some critical evacuation.[1]

In the bilious pleurisy or pneumonia of many regions of our country, the reader will recognize a disease much resembling the preceding, and which, unless biased by strong theoretical prepossession, he cannot fail to regard as the result of the combined agency of miasmata and atmospheric vicissitudes. The following description of the disease, by Dr. Eberle, embodies all that need be said on the subject. The initial symptoms differ very little from those which usually usher in an attack of ordinary remittent bilious fever. In some instances, a sense of fulness and tension is experienced in the right hypochondrium, a few days previous to the supervention of

[1] Dis. of Minorca, 261-266.

the disease, and occasionally dysenteric symptoms occur before the fever commences. In almost all the cases considerable pain is felt in the back and extremities during the premonitory period. The skin, from the beginning, is more or less tinged with bile, and the conjunctiva especially is conspicuously icterode. The face is flushed, and a sickly mixture of red and yellow, upon close examination, betrays the existence of a disturbed state of the liver. Acute pain in the forehead is almost constantly present. The pain in the chest is sometimes extremely severe and pungent; but more commonly it is obtuse, and attended with a sense of weight or oppression in the breast. In some cases the fever continues for several days before the pectoral pain supervenes. The expectoration is not very copious, the sputa being of a frothy, yellowish appearance, marked frequently with streaks of blood. The fever is generally attended with manifest evening exacerbations, and morning remissions. When vomiting takes place, an occurrence very common in this affection, more or less of bilious matter is generally thrown up, although in some instances the secretion of bile appears to be entirely suspended; the ejections consisting of nothing else than gastric mucus and the ingesta. The tongue is at first white, with a yellowish streak along the middle, which, as the disease advances, becomes dark brown and dry. The urine is always of a deep yellow or bilious colour; and the pulse is generally small, frequent, and quick, with a slight degree of preternatural tension.[1]

In the following statement we certainly do not recognize the phenomena of the ordinary and legitimate form of pneumonia, or pleuro-pneumonia, and must, as in the preceding, admit the admixture of some of the characteristics of malarial fevers. The vicinity of marshes within the tropics, and in countries, during the summer and autumn, subject to tropical heat, says Chisholm, is always influential in the formation of the type and character of the disease. This is remarkably exemplified in dysentery and pneumonia. In the latter it "gives the disease, originating in suspension or interruption of the functions of the cutaneous organization, a remittent or intermittent form. In fevers of an infectious nature, whether symptomatic or idiopathic, the same type is communicated by the reception of the marsh miasmata into the system, without suspending or changing the original diathesis. Thus smallpox, measles, and scarlatina have,

[1] Treatise on the Practice of Medicine, i. 284.

in such situations, a remittent, or even an intermittent form of symptomatic fever; and thus, from the same cause, idiopathic infectious fever has often superadded the type. All these, therefore, under such circumstances of locality, are truly hybrid diseases; although, in their treatment, the original disease is alone to be regarded. In hybrid pneumonia, the pneumonic symptoms and fever are concomitant, and affect the patient according to the type of the latter; in paroxysms, if it is intermittent; in remissions and exacerbations, if it is remittent; that is, the pneumonic symptoms are always exasperated during the presence of the fever, and mitigated during its intermission or remission."[1]

As we have seen in the opening chapter of this volume, Dr. Vaughan, of Delaware, founded his opinion regarding the identity—pathological and etiological—of pneumonia and fever, partly on the occurrence of cases of the former in which the pulse presented peculiar characters, the fever had a tendency to assume a tertian type, and the countenance became early hippocratic.[2] We have seen also that Dr. Forry inclined to the same opinion, because sundry cases reported by Drs. Pitcher and Wharton, of the army, partook of the intermittent character, and were successfully treated by the same remedies which are found to arrest intermittent fevers.[3] A farther proof of the existence of such cases will be obtained by referring to the great work of Dr. Drake, as well as to an essay by Dr. Boling, of Montgomery, Ala., in which the blending of the periodic type with pulmonary and other inflammations, is ably treated and amply illustrated by interesting cases.[4]

Within the last few years, as we learn from the *Charleston Medical Journal*, even thoracic diseases—bronchitis, pneumonitis, pleuritis, &c. (while retaining their distinctive characters), have taken on a strongly-marked periodical—intermittent or remittent—character. Two years ago, in Charleston, almost all the cases of the above-mentioned diseases were of this nature, and were promptly checked by quinia.[5] Nor is it in the south alone that such a tendency has been noticed. Dr. Rush, in his account of the bilious fever which prevailed in Philadelphia in 1780, states that in the spring of that year, a catarrh appeared among children, from one to seven years of age. "It was accompanied by a defluxion from the eyes and

[1] Manual of the Climate and Diseases of Tropical Countries, 106.
[2] Med. Repos. iv. 130.
[3] Climate of the U. S. 186.
[4] Am. J. of Med. Sc. viii. 87, and N. S.
[5] Vol. v. 824.

nose, and by a cough and dyspnœa, resembling in some instances the cynanche trachealis, and in others a peripneumony. In some cases it was complicated with the symptoms of a bilious remittent and intermittent fever. The exacerbations of this fever were always attended with dyspnœa and cough."[1] Among the sick labouring under the fever they had brought to England from Walcheren, or who were attacked after their return from the latter place, pneumonia was of common occurrence, especially about the month of November, when they were exposed to the combined influence of cold and humidity. In such cases the pulmonary influence was modified in its type and other peculiarities by the paludal fever with which it was associated.[2]

Sir George Baker, in his description of the influenza of 1762, states that the disease exhibited the intermittent character. "Sometimes it proved periodical, and of the tertian type."[3] Dr. Holland made the same observation in London, during the epidemics of that disease which prevailed there, from 1831 to 1838, remarking that the tendency to intermittent symptoms, both tertian and quotidian in type, and often very regular in period, seemed to be more common when the disorder was abating.[4] A similar fact was noticed by Dr. Rush, who, in his history of the influenza of 1807, observes: "The bilious fever which prevailed in August imparted to it several of its symptoms. There were obvious remissions and intermissions, great pain in the back, and apparent cessation of the symptoms of the disease on the third, and a return of them on the fourth day, &c. The disease appeared in one respect to be a monster; its head and breast wore the character of influenza, while its trunk and limbs indicated it to be a bilious fever."[5]

Pneumonia and other inflammations presented the periodic element, and other phenomena appertaining to malarial fevers, during an epidemic of the latter which prevailed at Auch (France) a few years ago, a description of which was given by an able physician of the place, M. Campardon.[6] Dr. Constant, who practises in one of the marshy districts of the Department of the Lot in France, has seen

[1] Works, ii. 231.

[2] Davis, Scientific and Popular View of the Fever of Walcheren, 10-42.

[3] Treatise, &c. in Collection of the Sydenham Soc. 73.

[4] Medical Notes—Connection of Certain Diseases, 64. Am. Ed.

[5] Vol. iv. 101.

[6] Bulletin de l'Acad. de Méd. viii. 634.

much of that form of disease, and draws attention to the signs which distinguish it. The initial shivering is more intense and prolonged than in ordinary pneumonia; the local pain is felt early, and always in front of the chest, although the congestion is localized posteriorly; it is more amenable to blisters than to leeches; violent headache comes on early, and is either frontal or sincipital; there is often severe lumbar pain, which observes the same stages of increase and decrease as the headache; the shivering is followed by intense heat, which, after several hours, gives place to abundant sweating; the pulse, during the paroxysm, in place of being full, strong, and vibrating, as in ordinary pneumonia, is rapid, soft, undulating, and compressible; there is never any purulent expectoration, these pneumonias never proceeding beyond the second stage, *i. e.* red hepatization, the pulmonary engorgement being rather a sanguineous congestion than inflammation; auscultation shows the rapid passage from the first to the second stage; for eight or twelve hours will be sufficient for the passage from a circumscribed râle to the hepatization of a whole side. The disease especially appears in summer and autumn, while ordinary pneumonia prevails in spring and winter. The blood from a vein is often below the normal temperature, very black, and deficient in plasticity. After rest, its surface acquires a bluish colour, especially if the patient is taking quinia. The clot is slow in forming, and soft. The buffy coat is absent, or very thin, and inclines to a bluish colour. In the district where the form of pneumonia here described is seen, purely inflammatory pneumonia is observed during winter; but, in proportion to the high temperature and the production of malarial emanation, this inflammatory element is replaced by the paludal one.[1]

In the foregoing instances we certainly have pulmonary inflammation; but the form which the disease assumes differs somewhat from that we see elsewhere. It is peculiar in its type, in the greater length and distinctness of its remissions, and in the gastric and bilious symptoms by which it is accompanied; as well as in the condition of the blood, pulse, skin, and expectoration. Everything, indeed, affords proof of the existence of a periodic or malarial element.

[1] British and For. Med.-Chir. Rev. xii. 552, from the Bulletin de Thérapeutique, xliii. 481–491.

The same complication is noticed on a large scale, both as regards pneumonia and pleurisy, in Rio Janeiro and other parts of Brazil, where, as we learn from Sigaud, who mentions the fact, the periodic or malarial element associates itself to all diseases. See his excellent work on the climate and diseases of that country, pp. 301–304.

Though sometimes encountered in ordinary fever seasons, and in most places subject to malarial fevers, and though more particularly rife in times of violent and wide-spreading epidemics, and in very sickly localities; this complication of inflammation of the lungs with miasmatic fever is not unfrequently found to occur under circumstances of a different kind; for, by virtue of the power of latency possessed by these, the system remains predisposed to them; and the same exciting cause which occasions an attack of pneumonia, long after or in a healthy season, calls into activity the malarial poison, and the result is the compound disease under consideration. This condition of things has been observed in all miasmatic countries, and nowhere more strikingly than in our own. By more than one of our southern physicians who have not adopted the views at present under examination, we shall be told, that, as many carry the predisposition to miasmatic diseases throughout the autumn without having fever excited, so some continue to retain it in the winter, and on some sudden exposure to cold, which often produces it in the autumn, fever is excited in those who continue in that state; or if the approach of cold weather be sudden and excessive, before the predisposition is done away, and proper provision made against the weather, the number affected is greater. In the winter following those seasons in which miasmata abound, the number of persons remaining predisposed, and the predisposition itself, are greater. Sudden cold, then, produces many cases of combined symptoms of summer and winter disease.[1]

But neither in these instances, numerous and interesting as they doubtless are, nor in those cases of remittent or intermittent peritonitis, rheumatism, ophthalmia, apoplexy, hemiplegia, convulsions, meningitis, lunacy, of which we read in the writings of Torti, Morton, Morgagni, Elliotson, Macculloch, Mongellaz, Gouraud, Laycock, Boling,[2] and examples of some of which I have myself not seldom observed in this country and Europe, can we discover any good reason for creating a new class of diseases, distinct from the ordinary inflammation, congestion, or irritation of the parts affected, both as regard cause and nature. Still less are we justified in admitting the identity of the two diseases represented in the compound of phenomena noticed, or that the one

[1] Cooke, Med. Recorder, vii. 460.

[2] See also Med.-Chir. Trans. iii. 348; Med. Gaz. iv. 116; Med.-Chir. Rev. xiv. 514; London Lancet, i. 425.

is a modification, or really and substantially nothing more than a peculiar form of the other. To me, as also to some of the writers just referred to, and to many more I might cite, all these cases furnish illustrations of the complication or coexistence of two distinct complaints, produced by distinct causes, having distinct seats and characters, and being governed by different laws, but which often modify each other to a greater or less extent.

Similar views were long entertained by high professional authorities. In an excellent essay on the "relation existing between epidemic and other diseases prevailing at the same time and place, and denominated intercurrent,"[1] Raymond, among others, in reference to facts he had observed during a long series of years, dwelled at some length on the complication of pneumonia with the epidemic or stationary constitution of the atmosphere existing at the time the disease happens to show itself, and remarked: "Those kinds of thoracic inflammations have constantly assumed the types of the epidemic constitution during the existence of which they appeared. They have engrafted themselves on the epidemic or constitution of the year, and presented the same symptoms and the same functional lesions, in addition to the affection of the respiratory organs which characterizes them in a special manner. Apart from the expectoration, which appertains to them, their march, their critical movements, and their mode of termination, were the same. Inflammation of the lungs is, therefore, composed of, or complicated with, the stationary modes or symptoms of the constitution or epidemic of the year, and of the transient and intercurrent constitutions of the period and of the seasons from which they arise." If, he continues, intercurrent diseases are founded on constitutional fevers, in their turn intermittent fevers are often complicated with the elements by which the former are characterized.

While the Walcheren fever which pursued the British troops to England, or attacked them after their return home, was the offspring of a morbid agent which had impressed the system in a distant land; the pneumonia with which, as we have seen it was often combined, was the effect of a cause appertaining to the place where the complaint showed itself. The combination thus produced must consequently be regarded as the result of the action of two seperate sets of causes, and hence as a compound of two inde-

[1] Mém. de la Soc. Roy. de Méd. iv. 72, 73.

pendent diseases. Dr. Davis, who calls attention to the occurrence in question, evidently takes this view of the subject. After remarking, that all the important consequences entailed by protracted intermittent, comprehended enlargement of the liver and spleen, wasting of the omentum, inflammation of the peritoneal cavity of viscera, extensive adhesions of the abdominal viscera to each other, scirrhosity of the pancreas, enlargement of the mesenteric glands, chronic inflammation of the intestines, dropsy, jaundice, &c. he adds: "The chief disorders combined with intermittent were pneumonia and angina. I have known pneumonia to be repeatedly combined with simple and double tertian paroxysms; and in Chapman's *Medical Commentaries*, there is a corroboration of pulmonary complaints assuming an intermittent type. Though it is a fact that autumnal intermittents are apt to run into dysentery, and vernal intermittents to attack the lungs, yet one of the most frequent combinations of the protracted fever of Walcheren was inflammation of the lungs, an affection that became very frequent in November, and that generally ended fatally." Section 8 of Dr. Davis's work is headed, "Pneumonia as a combination with the primary disease." He therein describes a peculiar form of the complaint, to which the fever patients were particularly subject, and adds: "Pneumonia never was symptomatic of the Walcheren remittent, as it sometimes is of measles, catarrh, and phthisis pulmonalis; but was combined and interwoven in a particular manner with it, so as apparently to constitute a part of the same disease."[1]

The true nature of these cases did not escape the sagacity of our great medical philosopher, Dr. Drake. "The lungs, it is well known," as he remarks, "are liable to inflammation in this fever; and, instead of occurring late in the disease, like cerebritis, it generally arises at an early period. Such inflammation may prove fatal; and then a *post-mortem* inspection will show the lesions resulting from bronchitis or pleurisy, but more frequently still those of pneumonia, such as sanguineous engorgements and hepatization. But they cannot be regarded as constant, essential, or characteristic of autumnal fever; for, *first*, a vast majority of cases, even those which prove fatal, do not present a single symptom of pulmonary inflammation; and, *second*, this inflammation, in most instances, is the undoubted effect of sudden changes of weather in the latter part

[1] *Op. cit.* 42, 149, 153.

of autumn, and must, therefore, be taken as the offspring of an incidental cause, acting subsequently to that which produced the fever." (824.) In another place, Dr. Drake states that the most frequent of the complications occasioned by the influence of malarial fevers is that presented by the pneumonias of the south, and also of the lakes in the north (765).

Again, in speaking of the complications of intermittent with other diseases, the same eminent writer farther says: "But the more frequent and formidable of these complications is that presented by the pneumonias of the south, as also on the shores of the lakes in the north, where numerous cases occur, which the profession too often find unmanageable by any method of treatment they have been able to devise." (i. 765.) In another place, he calls attention to the fact that the subdiaphragmatic viscera, except the pancreas, are subject to inflammation in remittent fever, and says: "Sometimes, however, from idiosyncrasy, or the co-operating action of other causes, inflammation in other parts occurs," "and, when the fever makes its attack late in autumn, the combined action of vicissitudes of temperature, and that of the specific cause, developed at an earlier period, may determine inflammation upon the lungs or pleura." (740.) "The pneumonia biliosa," says Dr. Potter, "is a compound affection, originating from a double remote cause." "It is the immediate offspring of a low temperature, engendered upon a miasmatic predisposition." (*Op. cit.*)

Dr. Eberle is decided in the opinion that this variety of pneumonia, which occurs during cold and variable seasons, abounding in sources of miasmatic exhalations, is the result of the combined agency of koino-miasmata, and atmospheric vicissitudes. (*Op. cit.* i. 284.) Other references to the same object, might be made, for there is scarcely a writer of note among us, or elsewhere, who has viewed such cases in a different light; but the above will suffice to show the opinion entertained on the subject by high authorities.

The lungs become implicated in a number of diseases; in typhoid and typhus fevers, the complication is of great frequency, and has been long recognized as such, as the writings of Huxham[1] and others will fully attest. This eminent physician informs us, that at Plymouth, in 1740 and 1745, abundance of people were seized with shivering, then great heats, fever, and difficulty of breathing; impor-

[1] Essay on Fever, 59–61; Obs. on Air and Epid. Dis. ii. 59.

tunate, laborious cough; very acute darting pains of the breasts, sides, and back; frequent also in the head and temples. They had oftentimes a very quick, hard pulse, but concentrated, as it were. The breath was very hot and offensive, and the matter they expectorated was sometimes thin and crude; sometimes as yellow as saffron, but more commonly a thin, gleety, bloody matter; frequently very fetid, and sometimes so acrid as to cause a great hoarseness and soreness of the windpipe and throat, and sometimes excoriations of these parts. The blood drawn from them was either of a darkish-livid colour, covered over with a lead-coloured or greenish thin film, or sometimes quite florid (particularly on the first bleeding), but of a loose, soft consistence when cold. In others, the blood drawn was covered over with a pretty thick tough coat, "not of a whitish-yellow colour, as usual in common pleuritic or pleuro-peripneumonic blood, but of a colour approaching to that of cornelian stone, or a little more dilute than that of the common jelly of red currants." The urine was commonly very high, and sometimes dark-coloured, with a kind of lead-coloured sediment; it was generally rendered in small quantities. Faint, uncertain, partial sweats often attended, particularly about the face and head; although many times, towards the fatal period, they were very profuse and colliquative. Livid or black spots frequently appeared about the state, and seldom or never failed of being the certain harbingers of death.

Huxham remarks that, at the same time when this malignant peripneumonia reigned at Plymouth and its neighbourhood, pleurisics, peripneumonias, and pleuro-peripneumonias were everywhere epidemic, and generally of the true inflammatory kind, arising from the cold, dry, northerly and easterly winds, which had for a long time prevailed. "Now I must farther take notice," he continues, "that contemporary with both these disorders, a contagious, putrid, petechial fever was very rife in and about this town, especially among the sailors and prisoners, and those that were very conversant with them; and it was chiefly among those sorts of persons that the malignant pulmonic fever raged, so that this seemed to be a complication of the common inflammatory peripneumonia with the contagious petechial fever; the contagious effluvia acting on the blood in the manner of acrimonious salts, and destroying its crasis." Montault[1] found inflammation of the lungs in twelve cases

[1] Mem. de l'Acad de Méd. vii. 209.

out of forty-eight of typhus. Louis found it in somewhat more than one-third of his cases.[1] Attention was called to the complication by Sauvages,[2] by Fodéré,[3] Chomel,[4] Bartlett,[5] Parr,[6] Low,[7] Hosack,[8] Wood,[9] and a hundred others.

In these cases, pneumonic symptoms are undoubtedly modified to a greater or less extent by the febrile poison, and the disease is not unfrequently wanting in the pathognomonic characters which serve to distinguish it when it exists alone. But in most cases the thoracic inflammation is easily distinguished, and is to all intents and purposes a pneumonia like every other pneumonia, but often, though not always, partaking largely of the congestive rather than of the purely inflammatory character; but whatsoever be its character, it is something superadded to the typhoid fever, and forming no essential part of it. In the disease which, since the days of Sauvages, has very generally been denominated pneumonia typhoides, we have sometimes a primitive or pure pneumonia, which, as it progresses, assumes a low or typhoid character. It is then constituted of common pneumonia and a typhoid or malignant state of the system. But in other instances, the disease presents from the outset distinct symptoms of both pneumonia and typhoid or typhus fever. It is therefore a mere complication. Such was found to be the case during the prevalence of the disease in various parts of this country from 1807 to 1820. As remarked before, typhoid or typhus fever extended widely and fatally. It was attended occasionally, though not necessarily, with pneumonia; sometimes with inflammation of other organs, and not of the lungs. The latter, consequently, formed no essential part of the disease; and when it occurred, was a mere complication. Our countryman, Dr. Wood, has pointed this out in a most satisfactory manner.[10] Dr. Hosack also well understood the nature of the disease. "You have," he says, "two opposite conditions of body to contend with—local inflammation on the one hand, and a typhus state of the whole system on the other. The causes of the disease are no less compounded than the disease itself. The local inflammatory affections are probably occasioned by the sensible changes of the atmosphere, while

[1] De la Fièvre Typhoide, i. 360.
[2] Nosologia, i.
[3] Méd. Lég. v. 351.
[4] Leç. de Méd. Clin.
[5] On Fever, 111.
[6] Dic. Art. Pneumonia.
[7] Med. Register, iv. 25.
[8] Med. Register, iii. 449.
[9] Practice, ii. 39.
[10] *Op. cit.* ii. 37.

the typhoid character of the disease is derived from an epidemic constitution of the air, the same which has given rise to the typhus petechialis, or spotted fever, which prevailed for some time past in our northern and eastern States, and which is doubtless the same disease as that now prevailing in Albany, with the exception that the present epidemic is complicated with the symptoms of local inflammation of the chest, brain, throat, &c., the effect of the present cold season of the year."[1]

The complication with relapsing fever was very common in Ireland in 1847.[2] Like symptoms indicative of pneumonic inflammation have often been observed in the plague, and pointed out by many writers from the days of Riverius.[3] They occur in several eruptive complaints, in dysentery, and even in anæmia. Why should it not be so? Assuredly, if the lungs are susceptible of taking on morbid action through the effect of certain causes, in persons previously healthy, they cannot help being more likely to be so when the system is labouring under disease; and when all the organs and tissues, as well as the circulating fluids are, consequently, more alive to the influence of morbific agents. Nor is it difficult to understand how the inflammation, by which they are attacked, can awaken into activity a fever, the cause of which had lain dormant in the system, and which, when developed, continues to prevail at the same time with the disease already in existence, and modifies it to a greater or less degree.

But these secondary attacks are mere complications; and it would be just as reasonable to regard, in Ireland and France, for example, where typhus and typhoid fevers are, as it were, endemic, all pneumonias which occur in individuals affected with those diseases, as due to the causes of the latter, and as being in consequence really and substantially nothing more than a peculiar form of them, as to hold all pneumonias observed in paludal districts and during fever seasons, and presenting some or many of the essential characters of autumnal fevers, as the offspring of the paludal poison, or as a mere form of that class of complaints. Inflammation of the lungs, therefore, when it presents itself in cases characterized by symptoms of periodic or autumnal fevers, is to be viewed in no other light than as a complication, and not as part and parcel of the fever in question.

[1] Obs. on Peripn. Typhoides, now prevailing in several districts of the U. S. 1813, Med. Regist. iii. 450.

[2] Dublin J. viii. 334.

[3] De Feb. Pestil. ii. 95.

Each disease may, and often does exist independently of the other; but both may, and sometimes do coexist in the same subject; and while in such cases the febrile disease is due to its specific poison, the pneumonia arises from the same cause that would under any other circumstances have produced it. In such instances of complication, each of the two diseases may be simply modified by, or as it were tinged with, the other, *i. e.* a violent pneumonia may present feeble symptoms of an endemic fever; or, *vice versâ*, to a severe attack of fever may be associated symptoms of slight pneumonic inflammation. In some cases they start together. At other times the one precedes the other; the former acting by enhancing the susceptibility of the system to the action of the cause producing the second; or calling into play morbid conditions which before had lain dormant or latent in the system. To this it may be added, that while in some instances the pulmonary inflammation is subordinate to the fever, and, so long as it has not reached beyond the first stage, disappears during the apyrexia of the intermittent form, to recur again with the accession of the fever; and while it very greatly abates with the subsidence of the hot stage or exacerbation in the remittent, as proved by the general symptoms and physical signs, and follows, as it were, the phases of its associate; in other and probably the larger number of cases, the disease only abates to a greater or less extent, but does not cease in the first-mentioned form, and does so but slightly in the latter, thus showing its independent existence. Finally, in such complications, it has not unfrequently happened, that the periodic fever has been arrested, while the local inflammation has continued to run its course, in the usual way, to recovery or death.[1]

The complication of diseases more or less distinct in their nature, and the modifying influence of epidemic over other complaints, are well known to etiologists and pathologists.—The fact of malarial fevers engrafting themselves on other complaints, and imparting to them the character of periodicity, is perfectly well known and generally admitted. It is one of such frequent occurrence as to surprise no experienced and observant physician, and illustrates while it confirms what has just been said relative to the complication of pneumonia with some or more of the phenomena of autumnal fevers. The latter,

[1] Torti, 371, 476, 490, 495, 496; Hippocrates on Popular Med. Hist. vii.: Morgagni, Letters, 31–33. See also Bailly, Mongellaz, &c.

as well indeed as other epidemic and wide-spreading diseases, have a tendency to impress their livery on every intercurrent complaint, however dissimilar in character; while they all, in their turn, not unfrequently become modified by other diseases that assume the epidemic garb and prevail in greater force. And I need scarcely remark, that such an effect can only be explained on the principle that such epidemic diseases, arising as they do from extensively diffused and powerful causes, complicate to a greater or less extent, and add a few or more symptoms to those appertaining to other diseases, by which individuals exposed to their morbid influences may be attacked.

The effect has been noticed from the earliest times, even from the days of Thucydides, who remarks that the epidemic fever of Athens obliged all other diseases to change their nature by assuming some of its symptoms. "And none of the usual endemic maladies made their attacks during its continuance; or, if they did, soon terminated in this."[1] It was pointed out in a particular manner by Sydenham, Hodges, Huxham, Storck, Lepecque de la Cloture, by our own Rush, and after him by other writers. See what takes place in regard to smallpox. If, in certain seasons, according to Sydenham and others, this disease prevails extensively, the fevers which appear at the time assume an inflammatory character, come on like the variola, have kindred phenomena—less, however, the eruption—the same disposition to sweat, salivation, &c. Thus, in the epidemic smallpox of 1667, 1668, 1669, the continued fever which prevailed at the same time assumed the characters of that disease, and was "not much unlike it, except in the eruption of the pustules, and the symptoms thereon depending." There was pain over the pit of the stomach; chills, headache, heat, absence of thirst; the same appearance of the tongue and urine; the same disposition to symptomatic and profuse sweat; to petechiæ, when the antiphlogistic treatment was not pursued; the same tendency to salivation which occurred in the fever when the inflammation was violent and accompanied the other disease when of the confluent kind. So alike, indeed, were the two diseases, in many of their phenomena, that "with the exception only of those symptoms which were the necessary effects and consequences of the eruptions, it was, if not identical, at least closely akin to the smallpox." "Finally," says S., "as

[1] Bk. ii. chap. 51. Bloomfield's Tr. i. 413.

the fever was most rife at that particular time when the ravages of the smallpox were greater in these parts than at any other time within the limits of my own observation, there can be but little doubt as to the identity of character between the two diseases." "For this reason," says Sydenham, after recapitulating the whole, "although no man dislikes the coining of new names more than myself, I may, perhaps, be allowed to designate the present fever as the *variolous fever*, from its likeness to the smallpox or variola." The practical indications were manifestly the same in both diseases, those excepted which the eruption of the smallpox and the symptoms thence arising afforded, and which could not be expected in this fever, because it was not attended with an eruption. The disease, though it affected fewer persons by far than the smallpox, nevertheless lasted as long; but in the winter, when that abated, this prevailed; and when the smallpox returned again in the spring, the fever went off, so as thus to leave the smallpox the predominant epidemic of the constitution of that period. The fever, notwithstanding, never manifestly ceased during this space, till at length it totally disappeared, together with the smallpox, in August, 1669. Notwithstanding these points of similarity, the fever could not be regarded as cases of smallpox without eruption; for, as Sydenham remarks, they occurred indiscriminately in all classes, though principally among adults, the greater number of whom had, in all probability, passed through the disease.

It may be useful to mention that, while this fever still survived, a diarrhœa, unaccompanied by any manifest febrile symptoms, was prevalent. It was worse in 1668. Sydenham says that at that time the constitution was tending towards the dysenteric, which was the character of the next coming years. He considered this to be the same fever with the variolous fever then rife, although it took another form, and exhibited itself with a fresh symptom. It was observed by him that chills and rigors preceded the diarrhœa. He also thought it evident that the diarrhœa itself depended on the same origin with the fever. Hence, he maintained that this fever arose from the inflammatory rays turned inwards towards the intestines, and so provoking them to repulsion. The blood in the meanwhile was freed by this division from those disorders which otherwise would have been caused by such rays. Besides this, the patient could not bear the pit of the stomach to be touched by the hand—a symptom common to both the fever and the small-

pox of this constitution. All this made it as clear to him as the light of day, that the diarrhœa was of the same nature and essence with the dominant fever; and this opinion was, he thought, confirmed by the effect of venesection, and the cooling medicine, regimen, and diet, which he found so advantageous in the fever. They cured the diarrhœa as well; whilst, if treated upon a different principle, it changed its character from a mild disease to a deadly one.[1]

The writings of Huxham contain accounts of several instances of similar complications. In 1729, in the month of July, the smallpox prevailed much at Plymouth; and, during this month, a slow putrid fever, which remitted towards the end, and at length intermitted, became very epidemic there. It chiefly affected the head, stomach, and loins, as if the smallpox was coming on, and was attended with an oppression at the breast, sighing, and great languor. Perhaps it was such a kind of disorder that Sydenham called febris variolosa. Children, young people, women, and the more infirm, were more especially attacked with it. The blood drawn from them was seldom viscid; the urine commonly thin, crude, with an ash-coloured, mucous, and imperfect sediment, as if made up of wheaten bran. The more perfect the sediment, the more hope. The tongue was not very dry, but daubed over with a glutinous brown mucus. Near the end of the disease a diarrhœa, and sometimes a bloody dysentery, was greatly urgent. These were fatal to some. But a kind of red petechiæ, red miliary, itching pustules, or large sweats, breaking forth, most commonly quite carried off the fever.[2] Something of the same kind occurred in 1740; the fever was modified by the smallpox. In 1745 and 1746 the contrary took place. The smallpox was modified by the typhus with which it was complicated.[3]

Diemerbroeck informs us that, during the prevalence of the plague in Nimeguen in 1635 and 1636, all the intercurrent diseases which showed themselves, assumed, before the close of twenty-four hours, the characters of the reigning epidemic; and during a whole year, he continues, *vix ullus morbus peste incomitatus visus fuerit.*[4] Sennertus observed, during the great plagues of 1616 and 1626, that nearly all other diseases disappeared; and whenever any of them appeared and lasted a few days, it was sure to become complicated with the

[1] Vol. i. 151, 152, 157, 158, and edit. of Sydenham Society.
[2] Observations on the Air, and Epidemic Diseases, i. 39, 40.
[3] *Id.* ii. 60, &c. 190, &c. Huxham on Fever, 3d edit. 131, &c.
[4] Tractatus, De Peste, 13.

prevailing fever. "The plague," says Pugnet, "reigns alone. In saying this, I do not wish to convey the idea that, when in all its vigour, it puts a bar to the development of all other diseases, but that it stamps its characteristic features on those that show themselves; assuming, itself, the forms imparted to it by the temperament of the individual attacked."[1]

A more recent and an excellent authority, who has often seen and studied the disease in Constantinople, remarks, that when the sickly season arrives, and the plague breaks out, it encounters, besides chronic diseases, those appertaining to the season, as well the sporadic as the endemic and epidemic. "If the plague is mild, the intercurrent complaints, and also those existing at the time, are uninfluenced in their progress. Sometimes, however, the regularity of their symptoms is disturbed, and we notice a concealed plague, or a painless exanthema. If the plague is malignant, its deleterious influence complicates a large number of the reigning or intercurrent diseases, arrests the course or changes the character of their symptoms, in order to substitute its own. It dries up blistered surfaces, issues, and setons; alters the character of the pus issuing therefrom, causes abortion, &c. If the epidemic is violent, acute and chronic intercurrent diseases almost invariably experience its influence, and the mortality among those affected is very large. In seeing the plague ingraft its symptoms on those of the larger number of prevailing diseases, the public fancy that, before showing itself with its legitimate characters, the former was concealed under the form of those various diseases, and was the sole cause of them. The complaint is pronounced to be *proteiform*, and people are no longer astonished at the errors of the physicians; while, in truth, the effect is only due to a deleterious influence added to the pre-existing diseases, and affecting the systems or apparatus of the economy with a force proportioned to the amount of their irritability.[2]

In speaking of the plague of London, Dr. Hodges remarks, that "at the rise of the plague all other distempers went into it; but that at its declension, it degenerated into others; as inflammations, headache, quinsies, dysenteries, smallpox, measles, fevers, and hectics, wherein the plague yet predominated."[3] Sydenham, whose observations extended over a period of sixteen years, including the

[1] Fièvres de mauvais caractère, 135. See also Rapport de l'Acad. 52, 58, 62.
[2] Brayer, Neuf Années à Constantinople, ii. 289, 290.
[3] Loimologia, 26.

time immediately preceding and following the great plague of London, calls attention to the fact that a remarkable change took place in the character of fevers and other complaints, approximating the general type of disease, in several striking features, to the distinguishing characteristics of the pestilence at hand, some months before that dreadful malady assumed its distinct and proper shape, which it did at last quite suddenly. He remarks that the disease which precedes the plague changes its character, and is accompanied with a multitude of anomalous accidents. "It is like a monarch with a body-guard of foreigners." Such deaths from plague as sporadically occur during the few years following those of the great plague, and which gradually decrease and finally vanish altogether, are to be attributed to a partial persistence of the pestiferous disposition of the atmosphere, which has not yet undergone its full change from foul to healthy. Deaths, in years like these, are only the gleanings of the harvest that has gone before; and it is from traces of former infection that the fevers of the first year or two after great plagues take a pestilential character; of true plague, indeed, they want some of the characteristics. Notwithstanding this, they resemble it exceedingly in character and disposition, and, as shown below, demand a similar line of treatment." (i. 99.) Again, in speaking of the epidemic coughs of the year 1675, he says: "And here I must again remark that, in the treatment of fevers, the physician who does not keep continually before his eyes the constitution of the year, the extent to which it favours the epidemic production of this or that disease, and the power it has of twisting to its own proper shape and likeness all the other concurrent disorders of the time, wanders widely in a maze without a clew." (i. 230.)

Dr. Rush, in his account of the Scarlatina Anginosa, which prevailed in Philadelphia in 1783 and 1784, says: "Intermittent fever, which made its appearance in August, was not lost during the month of September. It continued to prevail, but with several peculiar symptoms. In many persons it was accompanied by an eruption on the skin, and swelling of the hands and feet. In some, it was attended with sore throat, and pains behind the ears. Indeed, such was the predominance of the scarlatina anginosa, that many hundred people complained of sore throats, without any other symptom of indisposition. The slightest occasional or exciting cause, particularly cold, seldom failed of producing the disease."[1]

[1] Vol. ii. 215.

Dr. Southgood Smith has remarked that, during the six months immediately preceding the appearance of cholera in England, the character of fever in London so entirely changed, that typhus, which for a long series of years had been essentially an inflammatory disease, became a disease of debility, so closely resembling cholera, that the fever into which cholera patients commonly fell, could not be distinguished from the primary fever found in the wards of the fever hospital when cholera was at its height, which had appeared there for the first time six months previously, but which has never disappeared since.[1] So also in all our yellow fever epidemics, every intercurrent disease assumed to a greater or less extent the character, and presented some of the phenomena, of the reigning complaint.[2]

The same absorbing power—the same influencing agency, is ascribed by Desportes to the yellow fever of St. Domingo. Speaking of the epidemic which prevailed in the City of the Cape, in 1733, he informs us that the violence of the disease was such that it lulled all other diseases, and reigned alone. "This is the character of all contagious and pestilential diseases. Sydenham, and before him Diemerbroeck, had remarked it of the plague."[3]

Dr. Chisholm makes a similar remark in relation to the epidemic fever which prevailed so extensively at St. George, Grenada, in the year 1793. Most other diseases, he says, degenerated into or partook very much of the nature of this. Dysenteries suddenly stopped, and were immediately succeeded by the symptoms of the pestilential fever. Catarrhal complaints, simple at first, soon changed their nature; convalescents from other diseases were very subject to this, but it generally proved mild. Those labouring at the time under chronic complaints, particularly rheumatism and hepatitis, were also very subject to it. The puerperal fever became malignant, and of course fatal; and even among pregnant negro women, who otherwise might have had it in the usual mild degree peculiar to that description of people, were reduced to a very dangerous situation by it. In short, every disease, in which the patient was exposed to infection, sooner or later assumed the appearance, and acquired the danger, of the pestilential fever.[4]

Dr. James Clark states that, in 1793, children, adults, and old

[1] General Board of Health's Report on Quarantine, 13.

[2] Rush, iii. 76, 79.

[3] Maladies de St. Domingue, 40, 41.

[4] *Op. cit.* i. 180-182.

people labouring under smallpox, were attacked with the yellow fever about the time the secondary fever generally came on, whether the disease assumed the confluent or simple form.[1] Dr. Rush remarks that the bilious remittent, or break-bone fever, which prevailed in Philadelphia in 1780, chased away every other febrile disease, or blended itself with the intercurrent diseases.[2] In his account of the measles of 1801, the same eminent physician states that the disease wore the livery of the autumnal fever in the following particulars: "It was strongly marked by remissions and intermissions; the exacerbations came chiefly at night; there was in many cases a constant nausea and discharge of bile by puking."[3]

Dr. Balfour tells us that the intestinal remitting fever of Bengal, of which he has left us a graphic account, often appeared with symptoms of dysentery, rheumatism, and pleurisy.[4] In 1777, Clossot observed yellow fever associated with putrid typhus, and the union of two poisons in this way was noticed by Pringle and others in Europe, in the case of typhus and marsh fevers. The fever of Banker Street, New York, in 1820, was evidently a complication of bilious remittent and typhus—the idio koino miasmal fever of Dr. J. M. Smith.[5]

In the epidemic of Naples, in 1764, it was rare, as we learn from Sarcone, for other diseases, originating from causes different from those of the epidemic which scourged the city, not to pass finally into the latter.[6] Loew remarks that, during the petechial fever which prevailed in Presburg, in 1683, gout, colic, and sporadic diseases generally were often confounded or blended with the popular or epidemic fever.[7]

Dr. Blake, in his account of the climate and diseases of California, remarks that, although the influence of malaria does not show itself in the Sacramento Valley, "by producing any of the more marked forms of disease by which its presence is usually manifested, yet we have constant indications of its existence, by the character it impresses on almost every form of disease occurring in this locality."[8] The yellow fever which prevailed epidemically at Tam-

[1] Fever of Dominica, 19.
[2] Vol. ii. 235, iii. 77.
[3] Vol. iii. 73.
[4] A Collection of Treatises on the Effects of Sol-lunar Influence in Fevers, 124, 125.
[5] On Epidemics, 57.
[6] Maladies de Naples, ii. 59.
[7] Epid. de Morbo Petechiali, 5.
[8] N. O. Journ. ix. 510, 511.

pico, in 1836, exercised a marked influence on all the intercurrent diseases which, in consequence, presented themselves, clothed with some of the symptoms of the reigning fever.[1] Experience, indeed, everywhere shows that remittent, intermittent, and yellow fevers are sometimes engrafted upon or blended with smallpox, scarlatina, measles, dysentery, erysipelas, syphilis, scabies, hooping-cough, oriental plague, cholera, gonorrhœa, &c.

Dr. Nepple, in his description of an epidemic of periodic fever which prevailed in the Canton of Dombes, in France, in 1823, states that almost every febrile disease was then attended with periodical paroxysms, which subsequently degenerated into true intermittent attacks. Bronchitis, which was very common, and of a highly acute character during the winter, became complicated with nervous irritations, spasmodic dyspnœa of a more or less intermittent type, and with remittent paroxysms. Intermittent neuralgias were more than usually common. In a word, every disease appeared to have assumed a nervous and periodic character.[2]

The same phenomenon was observed at Auch some years later, where, independently of intermittent pernicious fevers which prevailed extensively, periodicity entered as an element in, or as a complication of the greater number of the intercurrent diseases, even in those which presented the inflammatory character, and in which physicians were more than once obliged to employ at the same time the lancet and the sulphate of quinia.[3]

The sweating fever prevailed epidemically in the Department of Dordogne, in France, during the greater part of the year 1841, affecting 10,803 individuals in a population of 83,342, and causing a mortality of 797, or 1 in about 5.5 of the sick. For two years previous, the Department (especially that part of it embracing the Cantons of Nantron, Riberac, and Périgueux), was overrun with eruptive fevers—measles, scarlatina, smallpox, and varioloid. Generally, one or two of these occupied the ground three or four weeks, and then made way for the others. In several instances, measles and scarlatina, or smallpox and varioloid, marched together; while in some, all four existed at the same time. Under these circumstances, the sweating fever made its appearance. In the course of

[1] Goupilleau, Rept. by Chervin, Bulletin de l'Acad. iii. 308.

[2] Tr. des Fièvres Interm. 137.

[3] Campardon, Aperçu sur les Maladies qui ont régné épidémiquement à Auch. Bulletin de l'Acad. viii. 684.

the year—from the 7th of May to November—the disease broke out five times in as many different places, and disappeared; so that the epidemic, considered in its *ensemble*, appeared, as it were, in five instalments. On four of these occasions it assumed suddenly its legitimate garb, and immediately drove away all other diseases.

On the fifth, however, things took a different turn. All at once, after a thunderstorm, on the 7th of May, and the days following, a disease heretofore unknown broke out. It was very different in its nature and results from the measles, which then prevailed; but yet difficult at first to recognize, owing to its being under the dependence of the complaint it so singularly and suddenly came to replace. The disease no longer presented exactly the same train of phenomena, the same kind of pulse, or eruption. Nevertheless, there was in that new physiognomy of symptoms a family air which greatly puzzled the physicians. The measles were modified. To the precursory febrile symptoms were added others which are strangers to the disease: while others which belong to it disappeared unexpectedly. Some days before, the patients had been seized with a chill, cough, coryza, and all the opening symptoms of measles. In some cases, one of these symptoms now gave way; in others, a different one disappeared; in all, the attack henceforward came on without the chill. Some, however, had a marked and well-defined rubeolic eruption; while in others the skin became covered with a miliary eruption.[1] To this interesting fact may be added another, somewhat akin to it, derived from high authority; that Storck and Lepecque de la Cloture mention epidemics of miliary fever, during which the pneumonias which prevailed terminated, not by expectoration, but by miliary eruptions, more or less abundant, or by fetid sweats.

Diseases arising from various species of malaria mix together and form compounds.—Furthermore, diseases arising from the operation of malarial effluvia, but having a separate and independent existence, marked by distinct characters, and governed by different laws, combine with each other and present groups of phenomena which, though they have given rise to considerable discussion and been subjects of angry controversy, must be viewed as the effect of such complications, and not as mere modifications of one and the same disease. The amalgamation of typhus with

[1] Parrot, Histoire de la Suette Miliaire. Mém. de l'Acad. de Méd. x. 395, 396.

intermittent fevers, diseases which differ materially on many points, but approximate on others, has been noticed in some of our Southern States, the cases exhibiting clearly the symptoms of the former disease; while, at the same time, the type was periodic.[1] Dr. Stoker, speaking of a number of cases of ague he treated in Dublin, some years ago, says that, "though the periodical revolutions which characterize agues were observable in these cases, still, they were more or less under the debilitating influence of the same epidemic constitution, and also of the same moral and physical causes which hitherto prevailed, and even at the same time impressed some cases of fever with the characteristics of typhus in an exquisite degree."[2] A like combination of true malignant yellow fever with simple or double tertians, or other varieties of periodic or autumnal fevers of various types, the existence of which was recognized and pointed out upwards of a century ago by Pouppé Desportes,[3] has been dwelled upon with more or less emphasis by scores of our contemporaries, and of writers of the preceding generation.[4] Its occurrence, the assertion of a few physicians to the contrary notwithstanding, is indisputable, and may be cited as a satisfactory illustration of the compounds in question; while the circumstance of its being overlooked or undiscovered may be viewed as one of the principal causes of the error committed by those pathologists who regard the former disease merely as a higher grade of the latter.

During the epidemic of yellow fever at Tampico in 1836, already referred to, intermittents, in particular, assumed this complicated form; for, during the continuance of the epidemic, they almost invariably presented, in addition to their legitimate symptoms, one or more of those appertaining to the yellow fever.

The yellow fever which closed its epidemic career in Charleston, a year ago, was accompanied by and blended with the breakbone

[1] Du Pré, on the Antagonism of Disease. Charleston Journ. v. 607, 608.

[2] Pathological Observations, pt. ii. 163.

[3] Maladies de St. Domingue, i. 230.

[4] Lemprière, ii. 70; Osgood, 27, 28; Imray, Edinb. Journ. lxiv. 337; Rochoux, 197; Nott, New Orleans Journ. iv. 586; J. M. Smith, on Epidemics, 310; Kelly, Amer. Journ. N. S. xiv. 376; Wood, Practice, i. 304; Chisholm, Letter to Dr. Davidson, Med. Repos. v. 231, 232; Lewis, New Orleans Journ. ii. 292, 413; *Id.* iv. 154; *Id.* v. 40; Charleston Journ. ii. 696; Dickson, Essays, 152, 153; *Id.* Trans. of Med. Assoc. v. 252; Barton's Rep. 30; Bryson, 70, 84, 196, &c.; Blair, 70; Heberden, 385; Williams, i. 303, 362, 464, 625; Desruelle on Hooping-cough, 12; Halphen, 1, &c.

fever, or dengue, and well-marked eruptions of several varieties—pustular, papular, and exanthematic. The complication of dengue with remittent and intermittent fevers has been noticed by Dr. Arnold, of Savannah,[1] and by Professor Dickson, of Charleston.[2] The like blending of remittent with typhoid fever is pointed out by Dr. Cain of the same city.[3] In a clever article on the Medical Topography of the Parish of De Soto, La., Dr. Gibbs speaks of cases of a mixed nature, which, "although affording all the usual indications of typhoid fever, displayed likewise the so-called malarial characters; as evinced by the distinct intermissions which attended for several days, the icterode appearance of the skin and eyes, and the yellow coating of the tongue, with other symptoms of bilious derangement."[4]

In Blair County, of this State, typhoid fever prevailed extensively from the middle of January, 1852, to September. Sporadic cases occurred in other months, but the disease only assumed an epidemic character during the above-mentioned period. Dr. Coffey, of Hollidaysburg, remarks, that the cases that happened in the summer and fall, resembled strongly in their commencement remittent fever, and for which the inexperienced readily mistake them. "Indeed," says the reporter, "the manifestations of this disease are protean. It occasionally simulates intermittent fever."[5] Typhoid fever is on the increase at Rio Janeiro, where, as we have seen, malaria abounds; and we are told by Dr. Sigaud, that cases in which the symptoms of the disease are combined with those of periodic fever, are very commonly encountered. To such an extent, indeed, does the complication prevail, that it at one time occasioned the greatest confusion in the minds of many as regards the diagnosis of the cases, and led to much hesitation respecting the proper treatment to be pursued.[6] Similar complications of typhoid with intermittent have been noticed in France.

M. Gauthier de Claubry, in his Report on the Epidemics of France during the year 1847, makes particular mention of the wide prevalence of typhoid fever that year; and states, as the result of observations made in various parts of the country, that sometimes the disease

[1] Charleston Journ. vi. 332.
[2] Trans. of Med. Association, v. 145.
[3] *Ibid.* 358.
[4] Fenner's Southern Reports, ii. 196, 197; see also Charleston Journ. v. 824.
[5] Trans. of Med. Soc. of State of Pennsylvania, iii. 75.
[6] Sigaud, Climat et Maladies du Bresil, 252.

presented the phenomena of intermittence or rather of remittence. This was often attributed to the influence of accidental causes, as marshes, stagnant waters, with the more reason, indeed, because this modification in the symptomatology of the typhoid affection occurred only in individuals placed directly under the influence of some of the above-mentioned causes, while the inhabitants of places situate far from marshes and stagnant waters, presented only the symptoms of ordinary adynamic or ataxic typhoid fever.[1]

Ruecker long ago recognized the complications of intermittent with malignant or typhus (probably typhoid) fever.[2] Pringle, on his side, regarded the morbus Hungaricus as a compound of bilious with hospital fever; and those who attentively examine the accounts we have of that disease, can scarcely refuse to admit that the opinion has the appearance of being well founded.[3] Indeed, it may be laid down as a fact of general occurrence, that when typhoid fever breaks out in paludal localities, after intermittents have there prevailed, the cases are frequently of a modified character, and assume a periodic type. And surely he would scarcely be listened to who maintained that all the diseases here enumerated, simply because they present themselves clothed with some or many of the symptoms of malarial fever, or impart some of their characteristics to these, must, therefore, be due to the cause giving rise to the latter, and are really and substantially nothing more than peculiar forms of one and the same complaint. An intelligent writer of one of our Southern States, after describing the several forms of fever of his neighbourhood, remarks, very pertinently: "Here are four varieties of acute disease, peculiar somewhat to certain terrestrial formations. Can any one possessed of candour and ordinary reasoning powers, examine into their history, and fail to see the striking characteristic differences. The enslaved and overtasked mind may reason, that, inasmuch as they are all fevers of the same season, appearing, blending, and mingling together, and often running into each other, specific distinctions cannot be made. To this kind of argumentation, we may properly reply, that red, white, black, green, are all colours. They may be so mingled and blended, running into each other by imperceptible degrees, as to produce various shades, and associate in the mind a most intimate and

[1] Mém. de l'Acad. de Méd. xv. 12. See also vol. xviii. 167.

[2] De Feb. Interm. Complicatione cum Malignâ Casa, &c.

[3] Diseases of the Army, 188.

inseparable connection; yet, when they are displayed in their primitive natural character, how boldly and prominently do they contrast."[1] Each of the diseases mentioned exists at times, and, indeed, generally, independently of the others; some are the offspring of specific contagious poisons, and can in no way possible arise from the operation of other causes producing different complaints; others are the offspring of non-contagious poisons; others, again, arise from changes in the sensible qualities of the atmosphere. They blend together in very many ways, in their types and characters, under the conjoint influence of several concurrent causes; and if, in their simple and uncomplicated garb, they must be held as idiopathic and independent disorders, they cannot cease to be so, when, to their own characteristic phenomena, are added some or more of those that appertain to malarial fevers. Now all this applies equally well to those cases which form more particularly the subject of our present inquiries; for in the same way that malarious fevers are sometimes engrafted on typhus, plague, &c., so they may be, and often are, engrafted on serous, mucous, and parenchymatous inflammations, and on that of the substance of the lungs among the rest. To say that such an occurrence is not possible, would be equivalent to maintaining, that a complication which undeniably occurs in regard to many diseases, even to some produced by specific and contagious poisons, cannot do so in reference to thoracic inflammations. I leave to those who feel disposed to hazard the assertion, the task of explaining the reason of this exception. If they succeed, we shall be forced to doubt the possibility of the coexistence of other morbid poisons in the same individual; for the evidence on which such coexistences rest is not a whit stronger than that adduced in favour of the complication of pneumonia with malarial fevers.

Diseases due to specific contagious poisons amalgamate together, or with other complaints, and form hybrid complaints, or exist together in the same subject.—Disprove satisfactorily the complication under consideration, and it will follow, that when writers, whatever be the respect usually accorded to their opinions and statements, tell us of the variolous poison being capable of coexisting with several other poisons—of its influencing their actions and being reciprocally influ-

[1] Lewis's Med. Hist. of Alabama, N. O. Journ. iv. 165.

enced by them; when they talk of smallpox existing in the same subject with scarlatina, hooping-cough, measles, miliary fever, psora, or syphilis; when they affirm that hooping-cough and measles, the vaccine poison and syphilis, scarlatina and measles, or scarlatina and hooping-cough occasionally combine together; when they cite cases in which typhus existed with erysipelas, scarlatina, vaccinia, psora, syphilis, or gonorrhœa; when they describe the plague as sometimes existing in combination with smallpox, vaccinia, syphilis, or cholera Asiaticus; or of dengue uniting with scarlatina—influenza with the latter disease or measles—or syphilis with herpes or psora; when, in addition, they inform us of three distinct diseases, as smallpox, measles, and hooping-cough; or measles, scarlatina, and chicken-pox, running their course simultaneously; when we are told that inoculation with a mixture of variolous and vaccine matters will produce, not, as Woodville stated, one or other of the two diseases, but *both;* when a case is cited on the authority of Leroux, in which the vaccine pustule was, as it were, imbedded in the variolous, and the matter of each, when used for inoculation, produced its specific disease; when, I say, they talk of such occurrences, we shall be justified in doubting the accuracy of their observations, and in maintaining that, instead of assemblages of distinct diseases, we have in all such instances really and substantially but one complaint, modified somewhat by peculiarities of season and weather, or other causes, but still to all intents and purposes the same.[1]

All these complications—all this livery wearing—all this amalgamation of things distinct from each other, is now perhaps too well known, and generally acknowledged, by those whose attention has been drawn to the subject, and who are, in consequence, best qualified to form an opinion upon it, to have required any lengthened remarks in this place, were it not that some writers among us and elsewhere, who are not backward in severely criticizing and casting ridicule on the views of physicians of high authority, and who charitably undertake to set the whole professional world right on

[1] See, on these various combinations, Nott, N. O. Journ. March, 1848, p. 586; Dickson, Trans. of Med. Assoc. v. 142, 143; Williams on Morbid Poisons, i. 40, 120, 211, 212, 264, 301, ii. 38, 65, 191, 296, 623; Holland, Connection of Diseases, 58, Am. ed. 64; Blair, 70; Heberden, 385; Fodéré, Méd. Légale, v. 352–357; Lafont-Gouzi, Materiaux pour servir à l'Hist. de la Médecine Militaire, &c. 47–83; Sarcone, Mal. de Naples, ii. 225; Anglada, Traité de la Contagion, i. 331, 332, 334, 336; Adams on Morbid Poisons, 11, 13; Bousquet, Traité de la Vaccine, 300; Robertson, a General View of the Nat. Hist. of the Atmosphere, ii. 370.

knotty points of pathology and etiology, appear to have lost sight, or to be ignorant of the true explanations of the occurrenees alluded to. If they have forgotten, they must be reminded, and if they do not know, they cannot fail to be benefited by the information, that the Hunterian maxim, which teaches that no two different fevers can exist in the same constitution[1]—a maxim from which Barthez started, in establishing his doctrine of the distraetion of forces,[2] and about which so much has been said—is unfounded; that though, in most cases, one of those diseases acquires the supremacy over the other, and either expels it or keeps it in check, in other instances, they progress simultaneously; that this takes place even in relation to eruptive fevers, a fact denied by Hunter; and that the whole of what has been remarked in the preceding pages is conformable to certain laws which should not be overlooked by those who undertake to describe the true and fundamental characters of diseases, and to assign to these their nosological position. When they come to investigate the subject as it deserves to be investigated, they will find that intercurrent, endemic, or even epidemic diseases, are influenced not only by the meteoric constitution of the period at which they appear, but that their characters, phenomenal and anatomical, are constituted at the outset of those appertaining to them, and also, to a greater or less extent, of those transmitted by complaints arising from anterior medical constitutions or reigning causes; and toward the close of their prevalence, of those characterizing succeeding complaints. They will find that such diseases seldom manifest themselves in their pure and unmodified garb except towards the middle of their career of prevalence, when the influence of anterior and succeeding constitutions or morbid causes is not felt; that what takes place in the same locality during different periods occurs at the same epochs in different localities—diseases which retain their purity of character in particular places, losing it as they reach other places where different morbid influences prevail; and that much the same results obtain in regard to diseases which attack individuals exposed to other morbid influences elsewhere, or in the same place at other seasons.[3]

[1] On the Blood, introd. 13. Am. ed.

[2] Nouv. Elem. de la Sc. de l'homme, ii. 181.

[3] See on these subjects Boudin's Geographie Medicale, 22, and Fuster's very excellent work, Des maladies de la France dans leurs rapports avec les saisons, 193–199.

"This fact," as Dr. J. M. Smith observes, "was specially remarked, by Sydenham and Hodges, of the disorders which preceded the plague of London in 1665. Similar instances are recorded by Bellinus,[1] Van Swieten, Bayley,[2] and, indeed, by most writers on pestilential epidemics. Dr. Mead sums up these facts in the general observation, that fevers of extraordinary malignity are the usual forerunners of plagues, and the natural consequence of that state of air which attends all plagues. Part 1, ch. 1."[3]

Pneumonia, like other inflammations, sometimes assumes a periodic type, independently of a malarial influence.—Most of the instances mentioned are doubtless the effects of the complication or blending of distinct diseases. But while admitting this to be the most proper explanation of the amalgamation of the phenomena they present, we must not lose sight of the circumstance that the inflammation of the lungs, like other acute affections, has a tendency in some cases to assume a remittent or intermittent type. That such a tendency may be noticed in that disease is a fact which we might *à priori* conclude; for there are but few cases in which symptoms of febrile reaction do not exhibit a diurnal abatement and aggravation more or less marked, or do not present every second or third day a more decided change of the same kind. As stated on a former occasion, the existence of a good and bad day is evidently recognizable in most cases of pneumonia; and I presume it may safely be averred that a decided remission is only a considerable diminution, and an intermission only a temporary cessation of the morbid condition existing during the abatement referred to. These remarks are equally applicable to other inflammations and irritations, wheresoever they may be seated, and by whatsoever cause they may be produced. This tendency to decided remissions and intermissions, occurs more frequently during the prevalence of the cause of periodical fevers, which modifies the morbid process, and imparts to it the element of periodicity, without, however, being sufficiently energetic in its impression, or meeting in the system with a sufficient degree of susceptibility, to produce a decided attack of fever, or even to give rise to the development of some of the more marked symptoms of the latter. But cases of the kind have occurred when,

[1] Webster, ii. 44.
[2] Fever of New York, 1795.
[3] Smith on Epidemic, 170, 171.

from the non-existence of periodic fevers in the vicinity, it was impossible satisfactorily to refer the effect to the agency of the admitted cause of such diseases, and under circumstances which precluded the supposition of its arising from the development of a morbid agent lying dormant in the system.

The manifestation of this tendency in pneumonia cannot, therefore, lend support to the idea of identity, as regards causation and nature, of that disease with malarial fevers, of which periodicity, partial or complete, constitutes a characteristic element; unless we are prepared to assert that all diseases in which we notice perfect or imperfect remissions—whether observed in districts of country subject to malarial complaints, or in places totally free from these—are the products of the same cause as remittent and intermittent fevers, and consequently really and substantially nothing more than peculiar forms of these. From this the strictest medical unitarian will doubtless shrink; for he must have seen—or if he has not, others have—cases afar from any malarial influence, in which irritation produced by mechanical or kindred causes, has been attended with, or followed by, febrile symptoms characterized by decided remissions, or, indeed, intermissions. Every one knows that the introduction of a bougie into the urethra has sometimes produced that effect. Bartholin, Forestus, Pollini, Rœderer, Mongellaz, and others, relate cases of intermittent diseases occasioned by the irritation of intestinal worms. Frank relates, from Sehmac, a case of the same kind, resulting from swallowing a piece of lard. Frank himself saw the same effect produced by mushrooms. A case fell under my own observation, some years ago, in which six febrile paroxysms were produced by the ingestion of a peach-stone. In this instance there could have been no malarial taint. Dr. Evanson, in his work on the *Diseases of Children*,[1] mentions a case in which febrile paroxysms of a remittent type, which lasted several days, occurred in a child, from swallowing a marble. The symptoms made their appearance soon after the occurrence, and were suddenly cured by the expulsion of the irritating cause. The same effect has resulted from the formation in the stomach of a cheesy coagulum, and was cured by its expulsion.[2] Dr. Pascalis describes intermittent paroxysms resulting from the irritation of decayed teeth; the elder Frank from a gumboil, &c. Other examples of

[1] P. 243, Am. ed.

[2] See Copland's Dict. ii. 1120; Stewart, Dis. of Children, 127.

the same import might easily be adduced; but the task is unnecessary; experience daily showing, that morbid irritations, by whatsoever causes produced, and in whatsoever organic apparatus or system of the economy situate, may, and often do assume the remittent and intermittent types. These are observed in irritation of the red capillaries, inflammatory and hemorrhagic; in sub-inflammatory and nervous irritations. Of all these, examples may be found in works of easy access. In a word, there are facts enough afloat, to prove that the element of periodicity does not belong exclusively to febrile diseases of a malarial origin.

So far from it, the periodical is as much a natural type as the continued. It characterizes many of the phenomena of health, and exhibits itself in the physiological play—both as regards progress and intensity—of many of the functions; in the processes of secretion, elimination, and calorification; in the operations of the nervous system; in muscular contraction; in the action of the heart, &c. Intermittence, indeed, may well be viewed as an element essential to the existence of the normal actions of the economy. What is more, it adheres to these actions in their passage from the state of health to that of disease, and may, therefore, be recognized as an element of this state also. It stands as an illustration of the great law of periodicity which regulates all the vital movements. This law has elicited, as we have seen, the attention of medical and physiological observers from an early period of our science, and has not been neglected in modern times. An American writer, the late Dr. Carpenter, of New Orleans, has, among others, pointed this manifestation of intermittence in the functions, and after showing, as Dr. Laycock had done already, that the periodicity of these is governed by a power or agency inherent in the system, and only in a secondary manner dependent upon the physical influences which surround us, properly remarks: "This principle once established, it becomes an easy matter to account for the intermittence of disease, by referring it to the persistent and controlling influence of these physiological oscillations, whose periods and intervals continue to mark and measure its stages and paroxysms. The matter of surprise and inquiry will then become, not why some diseases are intermittent in their course, but rather, why it is, that all of them are not so?"[1]

[1] New Orleans Med. Journ. iii. 423. See, on this subject, Bichat, Anatomie Générale, Lois d'Intermittence, i.; Holland on Morbid Actions of Intermittent Kind, Medical Notes, 193, Am. ed.; Mongellaz, Essai sur les Irritations Intermittents, vol. i. 3–5; N. A. Med. and Surg. Journ. i. 327.

Surely, when we take all these facts and circumstances into consideration, we cannot but perceive the impropriety of concluding, from the occurrence of remissions and even full intermissions in pneumonia, that the disease must necessarily be the offspring of the cause which gives rise to periodic fevers, and nothing more than a peculiar form of the latter.

The success of the anti-periodic treatment in pneumonia—supposing it true—no proof of the identity in question.—For these reasons, I am not prepared to admit that, in managing a case of pneumonic inflammation, "we must not lose sight of the fact that we are dealing with a constitutional disease—with a periodic fever—which is to be relieved by quinia, or to adduce, from the supposed beneficial effects of that remedy in the disease in question, an argument in favour of the identity contended for. This argument may be traced far back in the history of our science, when every disease that was cured by bark was supposed to partake of the nature of an intermittent fever—to be, in fact, only a periodic fever in disguise. The practical advice above referred to can, at most, apply to *some* cases only, and certainly not to all. In the large majority of instances of pneumonia, as observed everywhere—even in very many of those seen in the South—the physician will have no reason to bear in mind what Dr. Merrill lays so much stress upon. The disease is in no way connected with periodic fever. It is a constitutional affection in this sense only, that the system at large becomes implicated in the morbid disturbance by nervous and arterial reaction. In that respect it is closely allied to all other complaints in which important organs or tissues become, from some cause or other, the seat of inflammation, and in which, unless the powers of life are crushed, from the outset, and placed beyond the possibility of reaction, febrile phenomena set in. Hence, the principal object in the treatment will be, not to cure the constitutional disease, but the local inflammation and its various complications; because, on the disappearance of these, the case will come to an end. Supposing, now, that some cases get well under the use of quinia, the result could not prove the connection suggested. Before adducing that success for the purpose in question, it would be necessary, first, to demonstrate beyond doubt the reality of the benefit to be derived from the remedy, or rather of the superiority of the method proposed, if not the indispensable necessity of resorting to it; and secondly, to show

how, admitting the results to be as advantageous and marvellous as the warmest advocates of the method represent them to be, they can in any way serve to prove the dependence of thoracic inflammation upon ordinary periodic fever.

Now, as to the former of these points—the great benefit or superiority claimed for the quinia practice, when resorted to during the abatement of febrile excitement in pneumonia—I cannot greatly err, when expressing the opinion that many, very many practitioners, in all parts of the world, who have seen much of the disease, and acquired the needful skill in its management, will feel no disposition to join in singing the praise of that method, or to adopt it to the exclusion of the one they have heretofore employed. They will say, and I suspect that statistical returns will bear them out in the assertion, that while autumnal fevers, especially when they assume the truly periodic type, call, in some of their stages, for the use of anti-periodics, and more particularly of quinia, it is very far from being proved that the same will hold good with regard to pneumonia and kindred thoracic inflammations, which are, to say the least, cured just as well without as with the aid of the salts of bark; that in their usual form they yield to antiphlogistics, general and topical, and to revulsives, or even, in some instances, to the powers of nature; that, in the majority of cases, the quinia practice would be not only useless but hazardous, and should on this account be avoided; that, as a general rule, the remedy, if used at all, can only be so after other means of a different or opposite kind have been resorted to; and that, when these have been properly timed and judiciously employed, the disease seldom requires the aid of tonics, still less of those possessing an anti-periodic power. They will say, besides, that when tonics are called for, it is not in virtue of their anti-periodic effects; that as much advantage, if not more, is derived from other articles of the materia medica; and that, when the disease assumes the typhoid form, it may be more successfully treated by local depletions and revulsives, or by the latter without the former, together with stimulating diaphoretics and expectorants, and sometimes with stimulants and tonics. Finally, they will say that, when the latter are required or admissible, quinia will often answer a good purpose; but that it is doubtful whether it will be more serviceable than other articles of the materia medica. In all this, the majority of professional men throughout this country and in Europe will, I have little doubt, acquiesce.

In these parts, where, I should presume, the treatment of pneumonic inflammation is as well understood, and as successful, as it is in any other section of the world, I feel confident that few physicians of note would be disposed, from experience, to resort to quinia or other kindred remedies at any but the last period of the disease, or when signs of prostration or relaxation of the powers of life manifest themselves. But in such instances they would prescribe it on very different principles from those advocated by Dr. Merrill; and I think it might be shown that the same sentiment is entertained elsewhere, even in the very section of country where he has acquired the larger share of his experience.

To this it may be added, that the claim set up in latter times for quinia, of being a powerful and pure counter-stimulant or sedative—especially when administered in large doses—an opinion which, originating in Italy some thirty years ago, has received the support of several French, English, and American physicians,[1] is not, after all, sufficiently authenticated, so far, particularly, as relates to diseases produced by causes different from ordinary malarial exhalations, to justify a resort to the remedy in the treatment of pneumonia so long as signs of inflammatory irritation exist. Admitting, therefore, that all we have heard, and continue to hear, concerning the manifestation of that property, and of the wonderful effect resulting from it in periodic fevers, of all possible grades, types, and forms—to say nothing of dozens of other complaints, the number of which seems to enlarge with every succeeding writer, in some sections of our country—were placed beyond the possibility of doubt, the fact would not help those who, from a supposed similarity of effect of certain remedies in certain diseases, venture the conclusion that those diseases are similar; for it is yet to be proved, by correct clinical observations, that quinia exhibits that property, and is remarkably successful in pneumonia. Nay, more, supposing these wonderful effects, both as regards the latter disease and malarial

[1] Bailly, Traité des Fièvres Interm. 1825, p. 424; Guersant, Dict. de Méd. xxvi. 564; Geromini, Annali Univ. de Méd., March, 1841; Blair, 107; Drake, 746; Bell's Lectures, ii. 782; Boling, Am. Journ. N. S. viii. 89; Fenner, New Orleans Journ. v. 208: *Id.* 9, 318, &c.; *Id.* Southern Reports, ii. 849; McCormick, New Orleans Journ. ii. 175; Holmes, A. J., N. S., xii. 304; Merrill, New Orleans Journ. viii. 161, 163; Upshur, Stethoscope, ii. 437; McCaw, Stethoscope, ii. 666, &c.; Desiderio, Comptes Rendus de l'Acad. des Sciences, ix. 509; Bally, J. Gén. de Méd. Oct. 18, 1829, p. 7; Mérat et Delens, Dict. de Mat. Med. v. 607; Jacquot, Arch. Gén. 1845, vi. 76; Briquet, Traité Thérapeutique du Quinquina, 21, 42, 107, 123.

complaints, to be such as they are represented in certain quarters; supposing that quinia is a decided sedative; that it is a perfectly harmless remedy; that by its means fevers may be cut short at the outset; supposing that it may be safely, and, indeed, beneficially employed in large and repeated doses throughout the attack of these diseases, or before the complete cessation of febrile reaction, and that analogous effects may be expected to result, and are obtained, in pneumonia, it is difficult to perceive how, from these circumstances, a proof of the identity of this disease with fever can be derived.

Not only does the success of quinia in the latter fail to indicate that the same result must necessarily attend the employment of the remedy in pneumonia, but success in both complaints would lend no help to the advocates of the identity in question. In the first place, no fault can be found with those who in the present state of the question at issue, are skeptical as to the wonders related of quinia, and feel disposed to question the propriety of administering very large doses of it at the commencement of a febrile attack, without waiting for a marked remission, and especially during the continuance of high arterial action and symptoms of local inflammation or irritation; for, after all, this practice, for which Dr. Dundas[1] claims credit, but for which the profession in this country was long before his time indebted, some say (for this important point, if important it really be, has not yet been satisfactorily settled, there being several claimants in the field) to Dr. Thomas Fearn,[2] of Alabama; others to Dr. Metcalf,[3] of Miss.; and some, again, to Dr. Perrine,[4] of the same State; this practice, I say, though it enumerates warm and respectable advocates both on this and the other side of the Atlantic,[5] cannot be said with certainty to have proved, in the

[1] Sketches of Brazil, 287, 291, &c. [2] Fenner's Southern Reports, ii. 346.
[3] *Id.* i. 352. [4] Amer. Journ. xi. 250.
[5] Drake, Western J. xi.; *Ib.* Dis. &c. of the Valley, &c. 775, 789; May, Transyl. J. x.; Van Buren, Examiner, 1846; Upsher, *ib.*; Maillot, Fièvres Interm. 11, 140, 362; Broqua, Mém. sur le Sulf. de Quinine, Bulletin, vi. 619, 749; viii. 624; Fletcher, Med. Times and Gaz. April 23, 1853, p. 422; *Ib.* Braithwaite, pt. xxvii. 264, Am ed.; R. Gee and W. Eddowes, Lancet, Sept. 1853, p. 210, Am. ed.; Cummins, *ib.* 218; Sigaud, Climat et Maladies du Brésil, 245; Briquet, Traité Thérapeutique, 339, 358, 366; Tuck, N. O. J. ii. 303; Mitchell, *ib.* iii. 16; Merrill, *ib.* viii. 161; Fenner, *ib.* v. 209; *Ib.* ix. 318; *Ib.* Southern Med. Rep. i. 118; *Ib.* ii. 98; McCraven, N. O. J. v. 234; McCormick, N. O. J. ii. 173; Coolidge, South. Med. Rep. ii. 449; Haspel, Mal. de l'Algérie, ii. 332, &c.; Manson, Stethoscope, iii. 135.

hands of every good and safe practitioner, as advantageous, except perhaps under exceptional circumstances, as its originators and partisans so confidently assert.

To whomsoever the bright idea may have suggested itself among us, the practice is evidently but a revival of the one suggested and resorted to as early as the close of the seventeenth century, by Morton—adopted not long after by Torti, Burserius, Werlhof, Trnka, and other physicians of the times, and highly eulogized, so far at least as regards its applicability to yellow fever, by Arejula, Sarravia, Lafuente, Bobadilla, and the majority of Spanish physicians, as well as by Valentin, Cassan, Lefoulon, Guyon, Savarési, Stevens, Kuhn, &c.—of administering the Peruvian bark in the largest possible doses, in all stages of the disease, after little preparation, or without any preparation at all; and we all know that the success obtained from it, though satisfactory in a few cases, has not been such as to encourage its general adoption. The theoretical views upon which the practice was predicated were, doubtless, somewhat different from those by which the advocates of the quinia treatment, above referred to, are guided; but the effects claimed are similar. Forget for a moment that the writers cited are speaking of cinchona, and not of quinia, and you may fancy that the latter remedy is the subject of their remarks. Upon the administration of a large dose, it was said, the pulse is reduced; the skin cools and moistens; thirst, if it existed, disappears; the tongue becomes clean and moist; in a word, fever disappears as by enchantment; and if the patient experience a little uneasiness about the head, a little dizziness, perhaps a slight buzzing in the ears, the whole soon subsides, and convalescence follows. Such were the effects claimed for bark, in the class of disease in which the quinia, used in the way mentioned, is said to act as by magic. Is not the fate of the former practice calculated to raise some doubts as to the permanence of reputation of the latter?

Let it be said, *en passant*, that experience will doubtless one day demonstrate, to all whose eyes are not blinded by theory, that more noise has recently been made about the abortive plan of treating febrile diseases by means of scruple or even larger doses of quinia, given, with or without preparation, at the outset of the attack, with the intention of arresting their progress, than is warranted by the nature of the results obtained. Entitled as the authority of many

of the advocates of the plan may be to our respect, it may be fairly suspected that, in singing its praise so loudly, and proclaiming its superiority over every other heretofore pursued, they have allowed their imagination to get somewhat the better of their judgment. To this conclusion I am the more inclined, because it is yet to be proved that remittent and yellow fevers, when once established, can be arrested in their course; and the practice has failed, and even proved detrimental, in the hands of other observers, so far especially as regards the yellow fever.[1] Besides this, several of its more zealous advocates would extend the practice to every other form of fever named in the books—typhus, typhoid, &c.—in which, when resorted to by other equally skilful physicians, in this country and elsewhere, quinia, as an abortive, sedative, or specific, has failed to produce the anticipated effect,[2] unless perhaps the disease had assumed a decidedly remittent or intermittent type, when it required no prophet to tell us it would be useful. It is true that, by the warm supporters of the plan in question, no heed is taken of the opposition it has encountered; but on inquiry we cannot find that any stronger reason has been assigned (and, taking all things into consideration, it cannot but appear extraordinary that something better could not have been offered) for the failure experienced, even with what might well be regarded as classical doses of the panacea, than that physicians who recount their ill success would have obtained opposite results had they only given the quinia a fair trial; in other words, administered it earlier and in larger quantities. To those who have no hobbies to ride, the fact of these repeated failures, to say nothing of certain analogies they may bear in mind, and sundry theoretical views they may entertain respecting the pathology of the disease, and the mode of operation of the remedy, will be sufficient to deter them from joining in the hosannas sung in some quarters; while the allegation that southern and western physicians, who, we should think, cannot be accused of over-timidity in the use of reme-

[1] Stone, N. O. J. ii. 184, &c.; Dickson, Charleston J. i. 14; Lewis, N. O. J. i. 425, 427; *Ib.* iv. 174; N. O. J. x. 279; Furlonge, Lancet, Dec. 1853, p. 441, Am. ed. In this city, last autumn, the quinia practice failed completely.

[2] Gibbs, Fenner, ii.; Boling, N. O. J. ix. 2, &c.; Maggibbon, N. O. J. x. 25, 36; Scruggs, N. O. J. x. 206; Gordon, *ib.* 146, 210; Fletcher, Braithwaite's Abstract, July, 1853, p. 264; Grant, Am. J. xxvi. (N. S.) 104; Barclay, Med. Times, Jan. 8, 53.

dial agents—witness the history of calomel, tartar emetic, and the lancet, among them—have not been struck with the success of the abortive method, simply because they had not been heroic enough with quinia, which the most cautious among them daily use in doses which cause astonishment in excellent and skilful practitioners elsewhere, will appear passing ludicrous, and may recall to their minds, as it has done to mine, a certain passage in Gil Blas, in which the great Sangrado—the worthy prototype of more than one physician of an era not very remote from our own—accounts for the loss of the Canon Sedillot, whom he had, as a matter of course, bled profusely and deluged with warm water. I quote the original: "Comme il rendait les derniers soupirs le médecin parut, et demeura un peu sot, malgré l'habitude qu'il avait de dépécher ses malades. Cependant loin d'imputer la mort du chanoine à la boisson et aux saignées, il sortit en disant d'un air froid qu'on ne lui avait pas fait tirer assez de sang ni fait boire assez d'eau chaude."[1]

I am aware that the experiments of Bricquet and others show, that the sulphate of quinia, when given in doses of fifteen grains or upwards, has a tendency to reduce very considerably the frequency and force of the circulation; and that a similar effect is also obtained in lower animals, whether the remedy be administered by injection into the vessels, by the stomach, or by insertion into the cellular tissue.[2] I am aware, also, that this depressing action of quinia has been very frequently observed in this country and in Europe.[3] But while we admit all this, and feel convinced, besides, that many physicians, among us especially, have heretofore been too timid in the administration of the article, have used it in too small doses, and postponed it too long; that it is better to give it in a few smart doses, than in small and repeated quantities; and that, in some cases, it is tolerated, and has produced excellent effects in very large doses; nevertheless, it is impossible to shut our eyes to the fact

[1] Liv. ii. chap. ii.

[2] Comptes Rendus des Séances de l'Acad. des Sci. xxvii. 549; *Ib.* Reflexions sur l'emploi du S. de Q. à hautes doses, Bulletin, viii. 898; *Ib.* Traité Thérapeutique, &c. 21, 42.

[3] Briquet, Traité Thérapeutique du Quinquina, 21, 42, &c.; Bally and Banquier, J. Gén. de Méd. Oct. 1829, p. 7; Jacquot, Arch. Gén. 1845, vi. 76; Lambert, Essai sur la Méthode Endermique, 97; Guersant, Répertoire des Sc. Med. art. Quinquina; Legroux, Journ. de Méd. et de Chir. Pratiques, April, 1845; Boucher, Sur l'emploi du S. de Q. dans la Fièvre Typhoide. Thèse, 1846. See others, mentioned above, at p. 173.

that this sedative action does not always manifest itself; that quinia not unfrequently increases the force and activity of the circulation; that when the sedation is produced, it is for the most part secondary, and akin, as Guersant, McCaw, and others remark, to that occasioned by overpowering portions of alcoholic or other stimulants, or to the after effect of lesser doses of these; that it is preceded by a stage of reaction—in short, it may be, but too evident to be denied—that when produced, the depression of the circulation is obtained at the expense of the brain and nervous system (of which it is far from being a *true* sedative), and of the gastric and sometimes the entire mucous membrane, on which it operates as an irritant; that in ordinary, if not in all diseases, the existence of inflammation is generally found by physicians entitled to the fullest credit, to prove a bar to its production, and that this morbid condition is apt to be aggravated by the free use of the remedy. All this is so evident, that we may venture the remark that experience will, before a very long while, prove, to the satisfaction of all candid observers, that those who carry the vaunted abortive plan to the extent recommended; those who use quinia in very large doses, without regard to the existence of inflammation, and prescribe it as well during the exacerbation of febrile complaints as during the apyrexia, run the risk of doing much mischief; that they are recommending a method of treatment very far from being calculated to lessen the ratio of mortality; that of the cases which appear to have been greatly benefited, or cured by the use of quinia given in the heroic way mentioned, not a few have got well in spite of the treatment; that others are seriously injured by it, or receive the seed of much subsequent mischief; and that, of those who succumb under the plan in question, many might have had a better chance of recovery if treated in a different way.

The supposition must appear the more natural, when we reflect that more than enough may be gathered from experiments instituted in England, France, Italy, and this country, as well as from clinical observations made by most reliable authorities in all parts of the world, to demonstrate, beyond the possibility of doubt, that the sulphate of quinia possesses toxical properties of no inconsiderable power, and sufficiently glaring to prevent all prudent physicians from trifling with it, and from using it, without absolute necessity, in the enormous doses resorted to by a few French and Italian practitioners, but more particularly in certain parts of this country; often

without the most remote regard to the idiosyncrasy of the patient, to the condition of the stomach or bowels, and to other circumstances of like importance. Let the reader turn to the results of experiments on dogs and other animals, and he will find that, when given in large doses, quinia has occasionally produced stupor, dilated pupils, coma, convulsions, and death; and that dissection in such cases has revealed congestion of the brain and its membranes, and a fluid state of the blood. What is more to our present purpose, the experiments of Magendie, Mélier, and others exhibited, besides the phenomena mentioned, signs of congestion of the lungs during life, and complete hepatization of these organs after death.[1] Turning to the human species, we find that much mischief has sometimes resulted from the same remedy. Cases could here be referred to, in which an active quinia treatment, pursued with the view to cure what was supposed to be obscure remittent fever, gave rise to sundry and very distressing nervous symptoms, which it required much time to eradicate. I have now in my mind a case of that kind of fever, treated in strict accordance with the rules of the method in question, by one of its most zealous and experienced advocates, and with which I became conversant in the South; in which the patient, although she recovered from the disease, had a tedious convalescence, and remained for years a martyr to the effects of extra quiniaism. The remedy, when administered in large, sometimes in moderate, doses, has given rise, besides tinnitus aurium—its usual attendant—to agitation, headache, vertigo, subsultus tendinum, delirium, coma, hæmaturia, frequent micturition, dysuria, amaurosis, deafness, gastralgia, gastro-enteritic inflammation, diarrhœa, epileptic symptoms, extreme prostration, paralysis, loss of speech, uterine hemorrhage, numbness and coldness of the surface, ecchymosis, petechiæ, &c.

In all this, I am fully borne out by the results of the experiments of Briquet,[2] already referred to, of Bennet,[3] Mélier,[4] Desiderio,[5] Geromini,[6] Baldwin,[7] and others. Thus, the experiments of Desiderio show that quinia, when given in large doses, produces drowsiness, a difficulty of keeping the erect posture, a tendency to

[1] Mélier, *op. cit.* 726, 729. [2] *Op. cit.* xxvii. 549.
[3] Edinb. Monthly J. Jan. 1852; Am. J. Oct. 1852, p. 491.
[4] Mem. de l'Acad. de Med. x. 722.
[5] Comptes rendus de l'Acad. des Sciences, xx. 370. [6] *Op. cit.*
[7] Am. J. (N. S.) xiii. 292. See also Pereira, Mat. Med. ii.; Lancet, xxxix.

immobility, dimness of vision, and a dropping of the eyelids. Acetate of morphia and alcohol, when given in suitable doses (the size of these differing according to the kind of animals experimented upon), produce effects analogous to those resulting from quinia, and when administered in cases in which the latter has been freely given, add their effects to those occasioned by the other. On the other hand, distilled laurel-water produces effects of a contrary kind, and may, to a certain extent, be considered as an antidote to quinia. Bleeding is still more efficacious in that respect. Powdered digitalis appeared to produce analogous effects. The experiments of Briquet himself, upon which stress has been laid by the advocates of the heroic quinia practice, prove that, when thrown into the bloodvessels, the remedy produces cerebral excitement, and generally convulsions. When it penetrates the brain in an indirect manner, there is, first, a certain amount of general agitation; then follow debility, headache, vertigo, tinnitus aurium, and paralysis of the acoustic nerves, intolerance of vision, burning sensation in the orbits, weakness of sight, dilatation of the pupils, and blindness, numbness of the skin of the face, subsultus tendinum, trembling of the limbs, an appearance of intoxication; then a general collapse and loss of muscular power, diminution or cessation of pain in cases of neuralgia. Autopsy reveals injection of the large vessels of the pia mater, slight sandy injection of the substance of the brain, and sometimes meningitis.

The same experiments establish the fact that quinia produces an hyperæmic or engorged state of the lungs. In small doses it excites the lining membrane and secretory organs of the mouth, produces salivation, and increases the appetite and digestive powers. When given in large quantities, and persevered in for some time, it sometimes produces inflammation of the mucous membranes; and gives rise to vomiting, colic, diarrhœa, and all the phenomena of gastritis and enteritis in a mild degree. The urinary organs suffer irritation, as manifested by pain, frequent desire to make water, hæmaturia, dysuria, and even retention of urine. In women, the free use of the article not unfrequently gives rise to uterine hemorrhage. In men, small doses excite the sexual organs, while large and frequently repeated doses produce reverse effects. The skin is benumbed and cooled, and spotted over with ecchymoses and petechiæ. While doing all this—while producing these various morbid changes in the tissues and organs, quinia affects the compo-

sition of the blood, which, according to many,[1] it notably defibrinates, and renders fluid, dark, and incoagulable; or, as would appear to result from the numerous experiments and observations of Briquet,[2] it renders, on the contrary, richer in fibrin; and it would not be difficult to point out instances in which, when pushed to what certain physicians in our country would regard as safe and even moderate doses, it has caused death,[3] not only in animals experimented upon, but in the human species.

With these facts before us, it cannot be deemed improper to suggest the necessity of abstaining from administering the remedy in larger doses than are strictly necessary to insure its specific effect—which effect, in fevers, especially, is due not so much to the sedative as to the purely antiperiodic property it possesses. That this property, which the advocates of sedation have often confounded in theory with the latter is real, will not be denied by those who have seen quinia stop intermittent fever without producing any apparent effect on the system, save a little buzzing of the ears. In the next place, it is not improper to suggest the propriety, as a general rule, of desisting, except in cases of extreme emergency, from the use of it during the continuance of high febrile excitement, and especially during the existence of well-marked local inflammation, and of reserving it for the period when by other means an abatement or removal of these have been obtained; or for those instances in which the disease assumes a congestive or pernicious character, and must be put a stop to at all hazard. Let it not be forgotten that some of the most decided believers in the sedative property of quinia, who are friendly to its use in large doses in fevers—intermittent and remittent—and have tried it on a large scale, are decidedly opposed to its employment in those diseases whenever the surfaces with which it comes in contact—the mucous membranes of the stomach and bowels—are in a state of

[1] Magendie, Leçons sur les Phénomènes Physiques de la Vie; Mélier, Mém. de l'Acad. de Méd. x. 725, &c.; Giacomini, Annali Universali de Medicina, March, 1841; Baldwin, Am. J. xiii. (N. S.) 299; Guersant, Dict. de Méd. xxvi. 567; Bulletin de l'Acad. de Méd. viii. 905; Bonora and Arvedi, Ann. Univ. de Medicina, March, 1843, quoted by Mélier; Monneret, Mem. sur le S. de Q. à haute dose, June 27, 1843; Legroux, *op. cit.*

[2] Traité Thérapeutique, &c. 82, &c.

[3] Depuisaye, Examinateur Méd. 15th Feb. 1843, quoted by Mélier, 733; Briquet, Journal de Méd. de M. Beau, quoted by Mélier; Piédagnel, communicated to Mélier, Baldwin, *op. cit.* 293, 299; Guersant, *op. cit.* 568.

inflammation. Among these may be mentioned Briquet himself, who not only found, in his experiments, that this substance occasionally produces an inflammatory irritation of those tissues, but noticed that it invariably occasions injurious effects whenever the above-mentioned parts are already inflamed. Hence, he advises the discontinuance or omission of the quinia in all diseases in which those membranes are so affected. And as this condition, or at least a morbid state of irritation approximating to it, obtains very commonly at some period of remittent, and is not unfrequently encountered during the exacerbation of intermittent fevers, we may understand that, in the opinion of the great apostle of sedation himself, the use of quinia is very frequently contra-indicated in those diseases, and certainly should not be resorted to indiscriminately, and without due regard to the condition of gastro-intestinal surfaces.[1]

Be this, however, as it may, as regards autumnal fevers generally; considering—what every one knows—that, more than any other remedy, quinia finds a useful application in such fevers; admitting, for the sake of argument, that it really exercises a sedative action in them; that it may be both safely and advantageously administered in large doses during the exacerbation; and that the inflammation which accompanies malarial diseases is so modified by the poison that the system is enabled to tolerate, with comparative impunity, large doses of the medicine, nothing of the kind can be affirmed in regard to ordinary inflammations—that of the lungs among them; if not in a special manner. In these, quinia, in the hands of skilful observers, has not usually been found to exercise the pure sedative effect in question; and so far from generally proving beneficial, it has often, when administered during the force of the febrile excitement, exasperated the disease. Such is the case when quinia is given in the usual quantities; and surely, we have no reason to believe that the system, in such cases, is under the influence of a modifying agency capable of placing it in a condition to tolerate the medicine in the large and indeed immense doses we occasionally hear of.

It would be the height of impropriety, therefore, to argue, from the real or supposed benefit of quinia in autumnal fevers, that the remedy will be equally useful in pneumonia; and to conclude, from this similarity of effects, that the two diseases are identical in nature

[1] Traité Thérapeutique, &c. 265.

and causation, and that pneumonia is only a particular form of periodic fever. The impropriety of the conclusion will appear the more evident; because, while quinia is of immense benefit in periodic fevers, and may, as some aver, be used safely in them during the existence of fever, or even inflammation, it is found that it not only acts injuriously in pneumonia, but, when given in large doses, has at times exhibited a tendency to produce the evil we wish to remove. On this subject, the facts mentioned by Mélier (733, 734), and Baldwin (xiii. 299), can leave no doubt; while the reality of the tendency might, in the absence of those facts, have been foreseen from the results obtained on dogs and other animals, and to which reference has already been made. Nor is it less certain that the use of quinia in pneumonia has been discarded or its benefit doubted, even by some of the strongest advocates of its sedative action, and of its employment in the exacerbation of autumnal fever. Dr. Boling says: "As an antiphlogistic remedy in elevated and healthy localities (*i. e.* in which the disease is not complicated with or modified by malarial fever), it will probably never supersede the lancet, &c., though it may, in many cases, be brought to their aid."[1] "It may be administered," says Dr. Upshur, "during the intensity of the febrile paroxysm, but not if the fever is the result of pneumonia and arachnitis; and the physician who should give quinia in these diseases, to reduce the pulse, because he had seen it produce such an effect in remittent fever or rheumatism, would commit a great blunder."[2] Not different is the opinion of Dr. Lewis, of Mobile,[3] and other physicians of the South, who use quinia only in cases requiring tonics and stimulants, and shun it in truly inflammatory pneumonia.

A still later, and very intelligent Southern medical writer—a strong advocate of its use in the active stage of remittent, intermittent, and continued fevers—Dr. McCaw, of Richmond, Va., remarks: "Quinia does not apply itself to the cure of inflammation of a local character. It is not, in my opinion, an antiphlogistic at all." "I know that some of the quininists do say that it is of great use as a sedative, even in this class of diseases. I do not think so myself. I have not found it so, certainly. I have given it many times during the progress of pneumonia and pleurisy; complicated

[1] American J. viii. 110. [2] Stethoscope, ii. 437.
[3] N. O. J. iv. 174; ii. 629.

with intermittent and remittent fever, I have always seen it cure the complication, but never the inflammation. In truth, the short stimulating stage of the remedy would possibly add to the inflammation, but that its special influence over the accompanying fever, stopping the daily paroxysms of congestion which must be so pernicious to the favourable termination of the disease, amply repays you for this slight mischief."[1] "Notwithstanding the opinion of contra-stimulists," says a high authority, "it is admitted by unprepossessed physicians, that Peruvian bark (and its salts) aggravates pure acute inflammation, and it is hurtful in almost every parenchymatous and membranous inflammation. It aggravates pneumonia and gastro-enteritis with regular fever. It is not less injurious in inflammatory diseases farther removed from the centre of the circulation,"[2] &c.

Briquet himself, who as much as any one has insisted upon, and endeavoured to prove by experiment on animals, and the results of clinical observation, the sedative action of the quinia, and the necessity of using it in all febrile diseases, to moderate the action of the heart and arteries, acknowledges, in his very recent volume on the therapeutic employment of the remedy: 1st, that the coexistence of a large proportion of fibrin in the blood, or that of a severe inflammation of the membranes, and *especially of the parenchyma*, exercise on the heart an influence which the sedative property in question, administered in safe doses, cannot neutralize; 2d, that the administration of quinia, given in doses sufficiently large to insure its sedative effects on the circulation, produces in the economy a sufficiently serious perturbation to induce us to avoid the risk of it, in cases in which, from the severity of the disease, it becomes urgent to put a stop to it.[3]

The only instances of pneumonia in which quinia has been found undeniably and decidedly useful for the purposes contended for, are those in which the periodic element is marked by well-defined remissions, or complete intermissions, and the malarial complication is clearly exhibited. It matters not whether these intervals be of long duration, and exhibit the quotidian or tertian types, or whether the exacerbation or paroxysm return after a few hours, or even a single hour of repose—a phenomenon which we know to occur in

[1] Stethoscope, ii. 670, 671.
[2] Guersant, Dict. de Méd. xxvi. 581.
[3] *Op. cit.* 116.

other forms of diseases.[1] In these, and in every other case in which this periodic element is manifest, quinia should, undoubtedly, be resorted to, more especially when the disease displays a pernicious or malignant tendency—a circumstance which renders the recurrence of a paroxysm of the utmost danger. These cases, in which the inflammation is in all probability modified by the malarial taint, in such a way as to tolerate the use of remedies, which, under other circumstances, could not be borne with impunity, resist every other mode of treatment. They cannot be cured by antiphlogistics alone; for although by these the inflammatory affection of the lungs may, if uncombined, be removed, the malarial fever or taint is not to be so destroyed; and the recurrence of every paroxysm or exacerbation, has the effect of aggravating the local disease; which cannot, therefore, be eradicated, unless a stop be put, by anti-periodic remedies, to the complicating complaint. With the cessation of the latter, the pneumonic inflammation, if it has not reached beyond the first stage, generally disappears also. In other cases, it abates considerably, as indicated by an improvement in the general symptoms and physical signs. In others, again, it continues. When this occurs, it must be treated by the usual means. But these cases, which, as already said, are generally, if not universally, the product of a malarial influence superadded to the ordinary cause of the disease, afford us examples, not of simple pneumonia, but of positive complications of this disease with periodic fever, or of its modification through the agency of the febrile poison. As a natural consequence, they are rarely, if ever, encountered in our large cities, or in very many places where, though pneumonia prevails extensively, the malarial poison is not evolved, or exercises its baneful influence with little force, and during a short period of time only. On the other hand, they are of frequent occurrence in miasmatic regions generally, and nowhere more so than in our Southern and Southwestern States, where the quinia has consequently been used successfully. It cannot be surprising, therefore, that Dr. Farrar should remark, in his medical topography of Jackson (Miss.): "In this climate, a stage will usually be seen in pneumonia, when quinia may be used with freedom and efficiency, which for some years past has been my experience."[2] Others have written or spoken to the same effect.

[1] Mélier, Mem. de l'Acad. de Méd. x. 551.
[2] Fenner's Southern Reports, i. 357.

But in such cases, I repeat, we are not to recognize pure, but complicated and modified pneumonias.

In them, the disease receives, by virtue of its association with malarial fever, the periodic type by which the latter is characterized, and is benefited by quinia, not in consequence of the sedative or contra-stimulant action of this remedy on the local disease and the system at large, but of the anti-periodic power it possesses, and by which it is enabled to arrest the progress of the malarial, or periodic and complicating fever. The older physicians used bark freely in such cases, a point upon which the reader may refresh his memory by referring to the writings of Torti, Morton, Lauter, Alibert, Mongellaz, Gouraud, as well as to an essay on intermittent irritations, published in form of a review in an early volume of the *North American Medical and Surgical Journal*, by the author of the present volume. "There is no one among us," says Sarcone, "who is not in a condition to present numerous observations respecting the happy results obtained by cinchona in affections of the lungs, combined with periodic fever. Messrs. Serao, Ventapane, Rubertis, Cinque, Visoni, and others, have effected memorable cures with bark in diseases of the same nature, not only this year (1764), but also in the preceding ones. I myself, though confessedly young in the profession, and of little importance in comparison with men of eminent talents, such as those I have had the honour to name, am able to adduce numerous examples of the useful employment of that remedy in the diseases in question."[1] The use of quinia in the same disease is universally resorted to. I have myself employed it largely by the mouth or rectum, and have seen it employed by others both at home, in the south of this country, and in Italy, where instances of the kind are not uncommon. Of course, it is a matter of indifference whether the inflammation be in the lungs or elsewhere. Whenever it assumes a decidedly remittent or an intermittent type, and is blended with a malarial fever, or modified by a malarial taint, and more particularly whenever signs of malignancy or congestion manifest themselves, quinia should be resorted to, and that promptly. We must, as is well remarked by Dr. Merrill, take advantage of the first remission, otherwise we may expect the second exacerbation to become more violent than the first; an effect due, in part, to the fact that every violent paroxysm of fever—

[1] Maladies de Naples, i. 212.

more particularly when preceded by a chill—tends to aggravate the local inflammation or congestion, and prostrate the nervous power. Be this, however, as it may, cases such as have just been alluded to, are in a great measure exceptional, and their occurrence, as well as the treatment they call for, proves nothing, so far as regards ordinary and uncomplicated pneumonia, in which the quinia practice would stand but little chance of proving useful. Now this practice being found inapplicable to the greater number of cases of pneumonic and other inflammations, of doubtful necessity in others, and only decidedly important in a comparatively few instances of the disease—and those, too, in which the periodic type evidently arises from the complication or modification mentioned—the argument founded on the success or superiority of the mode of treatment in question in pneumonia cannot be sustained; for that usefulness or superiority, as regards the disease generally, is very far from being, as yet, proved. The first postulate must, therefore, be abandoned.

As to the second—that the success of quinia in pneumonia must be viewed as a sure proof that this disease is identical with, and constitutes only a particular form of periodic or autumnal fever, in the treatment of which that remedy is, if not a specific, at least a most efficacious remedy; it need only be remarked that, even were we disposed to recognize the validity of the claims set up in favour of quinia in all instances of pneumonia, it would be illogical to deduce from that superiority a proof of the identity in question. As every one knows, quinia is daily used advantageously in diseases which owe their origin to causes very different from the legitimate poison of autumnal fevers, and which, nevertheless, it is not probable any one would be disposed to regard as constituting really and substantially anything more than particular forms of those fevers. In articular rheumatism, arising without malarial taint, and having nothing in the world in common with autumnal fevers, it has been and is employed by Briquet and others, with, as it is said, great utility. No one but Dr. Macculloch will be inclined to maintain the intimate and *constant* dependence of neuralgia upon intermittent fever, and to regard it as being produced *exclusively* by malaria. That it is often, as remarked by Dr. Drake (863), the *consequence* of autumnal fevers, especially when it assumes the periodic type, no one will deny. But it is also found to be the consequence of other complaints in no way allied to such fevers;—cases occurring and assuming the intermittent character under circumstances

which forbid the idea of any malarial agency. Nevertheless, quinia is often beneficially employed in the various forms of that painful disease.

Whatever be the cause of periodicity in a disease, or rather whatever be the nature and cause of a disease, which presents a well-marked remittent or intermittent character, quinia will be found a useful remedy in its treatment. It is useful also in certain nervous disorders in which the periodic element does not manifest itself, and which have no more to do with periodic fever than with smallpox or syphilis; and if we conclude that pneumonia, generally, is really and substantially nothing more than a peculiar form of remittent and intermittent fever, and should take its appropriate place (in company with pleurisy, &c.) under the plain designation—*periodic fever*, on the ground that certain cases of it are, under particular circumstances, and at a particular period of their course, greatly benefited or arrested by quinia; if with Dr. Forry, and others, we admit that the subjection of these diseases to the same remedies which are found to arrest the course of the one, also arrests the course of the other, "implies a close alliance, if not a common origin;"[1] we shall be led to conclude, also, that all the other disorders in which it may be useful—wheresoever the locality, and at whatsoever season they may show themselves—are of malarial origin; and must, in like manner, take their appropriate place under the same plain designation. Such a mode of reasoning would lead us, if we wish to be consistent, to pathological deductions, at which our good sense must revolt, and which would ill accord with the principles of the inductive philosophy so dear to some of our opponents. No one will deny that mercury is the remedy for syphilis; for although, in the days of our infatuation for the Broussaian doctrine, many practitioners denied the necessity or propriety of that remedy, and attributed to it a thousand evils, experience has shown, and no less an authority than Ricord maintains, that it is superior to every other means, especially in the first stage of the disease, and that in many cases it cannot be superseded by any other. But mercury is found very useful, and even indispensable in various complaints: in hepatic and other glandular derangements; in inflammation of the serous membranes of the abdomen, chest, and head; in sundry diseases of the eye; iritis, for

[1] *Op. cit.* 185, 186.

example. It is useful also in various other inflammations and engorgements, and even in some forms of periodic and malignant fevers. Surely, we shall look in vain for a pathologist disposed to conclude that the benefit derived in these latter diseases from mercury indicates their identity with, or dependence on syphilis, of which mercury must be viewed as the specific. If such an admission cannot be entertained—if we acknowledge that the advantages derived from mercury, in the diseases mentioned, in no way justify a belief in the identity of these with syphilis, it is difficult to perceive the propriety of viewing pneumonia as nothing more than a particular form of periodic fever, on the plea that quinia may prove useful when resorted to at the period of remission. The second limb of the argument, founded on treatment, must, therefore, like the first, be set aside.

The hypothesis of the identity of pneumonia with autumnal fever not supported by facts and solid arguments.—But it is scarcely necessary to pursue the subject any farther. If what has been said is correct, neither the symptoms observed during life; the anatomical characters revealed after death in pneumonia and autumnal fevers; the treatment found useful; the mode of progression of those diseases; the localities in which they prevail; the external agencies by which they are influenced, nor any other point to which I have adverted, afford any support to the belief of their being pathologically identical, or of their arising from the same cause. The theory under examination must, therefore, fall to the ground, and those who uphold it stand chargeable with having ventured on a hasty and illogical conclusion. Indeed, they have built up, not a theory, but simply an hypothesis, resting on mere assumptions and conjectures. I say assumptions; for we look in vain in the writings of those who, discarding generally received opinions, have revived the hypothesis in question, for any facts or arguments calculated to sustain the position therein assumed. Thus, we are told by Dr. Merrill, in a passage already quoted, that, ac-according to his observations, "the pneumonias which prevail in this country generally—sometimes sporadically, and frequently as an epidemic—are really and substantially nothing more than a peculiar form of remittent and intermittent fever." In another page the writer says: "Let us talk as learnedly, and refine, discriminate, and vary our nomenclature as we may, to suit the fash-

ion of the times; when we come to deal with plain facts, as they are presented to us in practice, this whole class of diseases, to which I have here alluded (pleurisy, pneumonia, &c.), will be found to take its appropriate place under the plain designation, *periodic fever.*" This fever, it is maintained, is a general disease, affecting the whole system; but this general disease probably never exists without evincing a strong determination of diseased action to some particular organs. If the brain be particularly implicated, the disease assumes the shape of phrenitis; if the stomach, we have gastritis; if the liver, hepatitis; if the lungs, pneumonia, &c. All this, and much more of the same tenor, is affirmed as matter of fact. The decision, as it would seem, is without appeal; and all who refuse to acquiesce in it, run great risk of being stigmatized as having remained behind the times. But no pains is taken—no attempt made—to substantiate the particular opinions thus thrown out. Nothing is offered in their support; nothing calculated to show that the writer, who has hazarded them, is right, and all who have entertained, or continue to entertain different sentiments, are wrong. So far from it, we discover nothing but the bare statement; nothing having the most distant appearance of proof; nothing, certainly, so far at least as I have been able to discover, which may be viewed in the light of solid or plausible argument, based on correct and accurately observed facts, and calculated to satisfy sound pathologists and etiologists. For it cannot be supposed, after all that precedes, and in the present state of professional opinion relative to the reality and frequency of complications, arising from the co-agency of two or more distinct and independent causes, and of the successive evolution of separate diseases, that we shall admit, as a sufficient proof of the identity contended for, the circumstance that in some cases the symptoms of pneumonia are combined with a greater or less number of those of periodic or malignant fever, and *vice versâ;* that pneumonia coexists with, or succeeds to, these fevers; and that when the affection of the lungs, owing to such a complication, or to some other morbid agency, assumes the remittent or intermittent type, quinia will be found an appropriate remedy.

Let it not be said, in proof of the unity of all febrile diseases—the thoracic and other inflammations included—and of the identity of their efficient cause, that writers draw a line of distinction between two cases of disease, in which no one can detect the slightest

difference while they are in progress. Let us not be told, by our opponents, that "it may be a good employment for students in medicine to learn the distinctive character of each variety so invented and described, but when we approach the bedside of the sick, we find it difficult enough, and useless enough, to designate the precise nosological position to which each particular case belongs." Assuredly, the thrust here made at medical writers cannot have reference to the distinction they may point out between pneumonia and periodic fever; for it is scarcely possible to suppose that any one who has attended one month to clinical studies could confound them together so far as regards their phenomenal and anatomical characters. Allusion must be made to the line of distinction drawn between the various forms of fevers themselves. Now, I take upon myself to answer, that though some writers, as Copland, may have been guilty of too much refinement in their subdivisions of fevers, the fault is not universal; that in writings of deserved reputation, no greater number of varieties of that class of disease is admitted than can be proved to have an independent existence; and that, so far from there being any reason to conclude that there is not the slightest difference discoverable during the progress of cases between which writers draw the line in question, it is ten, nay, a hundred to one, that, when an identity between such cases is affirmed to exist, by the ordinary run of physicians, in opposition to the opinion of professional writers of admitted experience, and who, while having at their command the means of investigating the subject on a sufficiently ample scale, and in all its branches, are well trained in the art of conducting such an investigation, the cause of the opposition is to be found, not in the fact that the latter are really at fault, but in the inattention, carelessness, theoretical prepossessions or limited knowledge of the individual who undertakes to pronounce an opinion on the nature and proper nosological position of the cases placed before him.

The science of diagnosis is far from being one of the easiest to master. Indeed, the more we proceed in the study of it—the more we investigate, analyze, and compare the distinctive features of diseases, their pathognomonic phenomena, their points of analogy and dissimilarity—the more satisfied must we become, that it is not every physician who can safely be entrusted with the task of discriminating between the various forms of febrile complaints, properly so called; especially if these present, as they sometimes do,

phenomena of a somewhat approximative character, or blend with each other in the way already mentioned. Errors are often committed, especially at the outbreak of an epidemic, even by those who have made a particular study of this branch of medical knowledge, and enjoyed the advantages afforded by an extensive field of observation in public and private practice. They must, necessarily, be more frequently so by the less informed, expert, experienced, or careful members of the profession, or by those who do not enjoy the opportunity, or whose time is too much engrossed to prosecute the study of pathological anatomy and other branches of medical knowledge, without which it is in vain to pretend to accuracy in diagnosis. Individuals of this kind necessarily abound in every community, though nowhere perhaps more than in this country. Many, hence, may be very apt to see identity, where more experienced, accurate, and trustworthy diagnosticians and pathologists will detect diversity. The physicians of Charleston find no difficulty in distinguishing the yellow or stranger's fever from the ordinary endemic remittent of the adjacent country, which by some has been, without due attention to distinctive phenomena and other important circumstances, confounded with the former. The same may be said of other diagnosticians and experienced observers in various parts of this country, in the West Indies, on the coasts of South America and Africa, and in Europe, who draw the line of demarcation between those fevers with undeniable precision.

The best French and English pathologists, as well as many accurate and sound observers in this country, have learned to draw a proper distinction between typhus and typhoid fevers, and so on of other febrile complaints; and they would probably demur to the opinion of those who venture to maintain that typhus and typhoid fevers are identical with bilious, remittent, and yellow fevers. In fact, the farther knowledge has advanced—the more closely the phenomena of the diseases mentioned have been analyzed and compared—the more their mode of progression has been scrutinized and their anatomical characters minutely and accurately studied, the greater has been the tendency among medical men to draw a precise line of distinction between their several forms. So great, indeed, has been the change in that respect—such the increase in the number of its advocates in quarters where minute and correct observations are alone to be looked for, that we cannot err greatly, when we express the opinion that the day is not far

distant when physicians generally, regardless of the clamour of a few opponents, will feel no more disposed to jumble up together the different varieties of fevers, as is still but too often done by a certain class of physicians, than they do now to regard, as did our ancestors, smallpox, measles, and scarlet fever as one and the same disease, really and substantially.

That difficulty is experienced in making out the diagnosis of fevers, especially in the commencement of an attack, no doubt can exist. The difficulty is often felt, even by experienced physicians, in the case of diseases, the nature and causes of which differ essentially, and which approximate only in being attended by symptoms of constitutional excitement. On this subject I need hardly insist. It is, or certainly ought to be, admitted by all who have the least smattering of practical medicine. If this be true in reference to such diseases, the embarrassment may well be admitted to occur much more frequently when the complaints to be diagnosed are of the same family, though of different species; for all know that in such diseases the full development of the phenomena which serve to characterize the case, is usually preceded by febrile symptoms very similar in all; and well calculated, on that account, to create difficulty in respect to the diagnosis. If we take, for example, the incubative symptoms in continued fever, we shall find that the description of them may apply to many of the acute blood or zymotic diseases.

The following catalogue of symptoms I borrow, like Dr. Simons, from Dr. Watson's matchless book on the practice of medicine. "The expression of the patient's countenance alters; he becomes pale, languid, and abstracted; those about him observe that he is looking very ill. He is feeble, and easily tired; reluctant to make any exertion of mind or body; listless, and often apprehensive of some impending evil; he loses his appetite; his tongue becomes white, and inclined to tremble; the bowels are irregular, often confined, sometimes affected with diarrhœa; his senses lose their natural delicacy. He has uneasiness or wandering pains in various parts of the body, and occasionally there is some giddiness; drowsiness, perhaps, during the day, and unsound, unrefreshing sleep at night. In a word, the patient droops. The regular onset of the fever is very frequently, indeed, marked by a shivering fit; another common phenomenon at the period of the invasion is severe headache. But you will also perceive, even when there have been no

premonitory circumstances, that symptoms arise, even thus early, which belong to the nervous system, and denote some disturbance and alteration in the functions of sensation, thought, and voluntary motion. They are comprised under the general phrase, 'febrile oppression,' and they are different from what we notice when pyrexia or feverishness supervenes upon inflammation. The muscular power is sensibly enfeebled. Sometimes the patient will struggle against this, but in a few hours, or in a day or two at farthest, he takes to his bed."

To these symptoms, which, as Dr. Simons remarks, constitute equally a picture of most of the diseases of the class, there succeed a stage of reaction in which fever runs more or less high. The skin is hot and dry, sometimes moist; the pulse is excited, thirst is developed, and so on of other symptoms familiar to all physicians; and it is only after these have continued more or less time that the characteristic phenomena manifest themselves, and the true nature of the case is satisfactorily made out. Exceptional cases, doubtless, are found, in which from peculiar signs, aided by various concomitant circumstances, the diagnosis may be established earlier. But in general it is not so; and every prudent physician finds it better to avoid precipitancy in the expression of his opinion. With Dr. Nott, therefore, we must all concur, when he remarks: "If a physician were called in the forming stage of a number of cases of the plague, smallpox, yellow fever, some forms of typhus, and other diseases arising from morbid poisons, as well as certain vegetable poisons, he would be much at a loss how to distinguish them for two or three days; and in some of those in which the characteristic signs are never developed, as smallpox without eruption, &c., a diagnosis never could be made. It should not be wondered at, then, that difficulty of diagnosis should sometimes occur between bilious and yellow fever, which belong to the same family, the same season, and (often) the same locality."[1] Another cause of difficulty in the diagnosis depends on the occurrence of the complications already adverted to; for it is a circumstance well understood by observant and experienced pathologists, and which has not escaped the notice of some of our distinguished Southern physicians, that when two or more epidemic or atmospheric diseases prevail together, they become blended. Under circumstances of the kind,

[1] N. O. J. iv. 584.

yellow fever, especially if it prevails in a mild form, and does not, by the wide diffusion and great energy of the poison giving rise to it, take exclusive possession of the field, becomes more or less blended with remittents and intermittents of all grades, and forms with these compounds, which may sometimes embarrass the most correct diagnostician, and are sure to puzzle and confuse those less expert in matters of the sort, and to lead them to doubt the propriety of establishing a line of demarcation between those fevers.

The physician who would be astonished at the appearance of black vomit, in a case of what he fancies to be ordinary fever, and who would always wait till the occurrence of that formidable symptom before establishing his diagnosis and pronouncing the disease to be yellow fever—who could not know the latter to be such till the patient was *in articulo mortis*—should be advised (supposing the case to have been really of the kind mentioned—for every one knows that black vomit alone is not sufficient to characterize yellow fever) to go back to school, or to keep his eyes wider open the next time. If the appearance of black vomit were indispensable to enable the physician to establish his diagnosis, it would follow that, in a large proportion of cases of what there can be no doubt is yellow fever, the true nature of the disease could not be positively ascertained. As a general rule, it may be stated that, in cases that recover, black vomit does not make its appearance. In this city, the mortality among the reported cases has averaged one in 2.12, the proportion varying from one in 1.2 to one in 3.86. In other places, the loss has occasionally been less. The number of those who escape that symptom must hence be everywhere larger than that of those who suffer from it. The amount of the former will be found to be even greater than here stated; for those who die do not all eject the fluid, and it then requires an autopsy to ascertain that it has been effused. And yet no difficulty is experienced, in those who recover, or die without throwing up the black matter, in distinguishing the disease from other forms of febrile complaints. Such errors are generally the result of want of skill or want of attention on the part of the observer; for the yellow fever, as also the remittent, present, together with phenomena approximating them to each other and to different complaints, characteristic features of their own, which, when duly and carefully examined and analyzed, prevent the one from being mistaken for the other, except in cases of an anomalous or complicated nature, or at the

very outset of the attack—of mild cases especially—or which, at any rate, enable the physician to establish his diagnosis, in the ordinary forms of the disease, long before the accession of that formidable symptom. The blunders of the unskilful or careless, or the fancies of the unitarian must not be urged in denial of the possibility of tracing a line of distinction between the different forms of autumnal fevers. Let them, by way of encouragement, peruse the following statement made by a clever physician of Mobile: Two clergymen of this city, Mr. Balzan and Mr. Dorman, whose active benevolence has won for them much correct information in relation to fevers, have frequently, in my private and hospital practice, designated each variety and grade of fever with the greatest ease and correctness, separating the grave from the ephemeral cases of yellow fever, and distinguishing these again from periodic fever. In their diagnosis they were governed by the character of fever, pain, restlessness, colour of skin, physiognomy, paroxysms, and nature of the secretions.[1]

I have not unfrequently heard it affirmed by physicians of the South, who favour the opinion under examination, that the identity contended for is shown by the fact that men digging down a bluff will often become sickly, and have fever with periodicity. Physicians who attend them report some to be affected with hepatitis with complication of pneumonia, and the reverse; others to have gastritis complicated with phrenitis, and the reverse; enteritis, nephritis, &c., with their complications; remittent bilious fever, intermittent fever, yellow fever, &c. &c. Now, it is argued, all these men derive their sickliness from the same cause—the digging down of a bluff, &c. If one and the same cause can produce so many groups of morbid phenomena, does it not seem almost certain that the disease thus engendered, though designated by as many as forty different names, is essentially the same in all cases?

For my part I must, with due deference, demur to this conclusion, and express the belief that I shall be fully sustained by many well-informed and experienced physicians on this and the other side of the Atlantic, when I deny the possibility of so great a variety and diversity of well-marked effects resulting from the agency of one and the same cause; or of the same disease assuming such a diversity of forms as to *simulate* as many as forty separate diseases,

[1] Lewis, Fever of Mobile in 1847, N. O. J. v. 40.

each entitled to a separate name, and occupying a separate place in our nosological arrangements. If it be found that a number of men, employed in cutting down a bluff, are seized with a diversity of what we are accustomed to regard as distinct diseases, and that these are all referred to the same cause; or if we are told that the disease in all these cases is the same, but has assumed a diversity of aspects, and merely presented itself in a variety of groups of symptoms, which, though having in many instances little or no resemblance to each other, are nevertheless the offspring of one and the same parent, pathologists will, in all probability, be disposed to accuse the reporters with betraying a great deficiency of knowledge, both as regards the nature and causation of the morbid phenomena described; or with an unpardonable degree of hastiness and carelessness in the manner of making their observations and drawing their conclusions; or perhaps with being under the spell of some preconceived notion or favourite hobby. The digging of no ditch or canal, the cutting down of no bluff, the levelling of no streets, has ever produced such diversified complaints; and when we hear of physicians giving many names—no matter whether ten or forty—to the morbid phenomena resulting from the agency of exhalations issuing from the upturned earth, we may safely set them down as erring; and as having, owing to their inability to establish a correct diagnosis, regarded in the light of distinct diseases what other and more careful observers would have found to be otherwise. On the other hand, when several diseases *are* really found to occur among men thus similarly employed—a circumstance not unlikely to occur—it is much more natural to conclude that different causes have been simultaneously at work, and that each has produced its legitimate effects, than to attribute those diseases to the same agent, which, from our experience elsewhere, and under different circumstances, we have reason to think are incapable of giving rise to such multitudinous and diversified phenomena. Such diggings and cuttings produce no catarrh, no pleurisy, no pneumonia, &c.; and if those diseases occur among the diggers and cutters, there is no reason why they should not be occasioned by the causes that produce them elsewhere; as well where fevers prevail, as where they are not observed; as well where the fresh earth has been recently exposed, as where it has not been disturbed. From such operations, when undertaken in particular seasons of the year, in certain localities, and within certain degrees

of terrestrial altitude, diseases, sometimes of a formidable character, undoubtedly result. This was exemplified in New Orleans at the time of the opening of the Carondelet Canal, in 1794–1797; in the same city, during the extensive paving executed in 1817, 1824, 1832; and at the opening of the Bank Canal, in 1832–1835. It was also exemplified in Natchez, at the period of the levelling of the streets, in 1816, 1825; at Memphis, Tenn., within the last few years; and, indeed, as already seen, in every place where canals, ditches, and other excavations have been made, and the earth extensively upturned. But in all such instances the disease produced has been fever; often of a malignant, at other times of a remittent form; according to the season of the year, the peculiarities of the weather, the nature of the soil, and other influencing circumstances, which it is needless to enumerate.

I am not ignorant of the fact that, by many physicians who entertain sentiments different from those expressed in the preceding pages, little or no respect is paid to the opinions of professional writers or teachers, however eminent these may be. Few of my readers can have failed to hear such authorities unceremoniously stigmatized, by those who throw out some new or revive some antiquated and forgotten notion, as being a long way behind the times, and as having not yet renounced the Aristotelian, and fully adopted the Baconian philosophy. It is not uncommon, to hear it affirmed that the whole system of pathology and practice, etiology and physiology of fever and febrile affections, is founded upon preconceived notions, and not upon facts and inductions, and that it requires to be remodelled on more correct principles. All this, and much more of similar import, we are not unfrequently told in ephemeral publications, and even in works of higher pretensions. I have neither room nor inclination to examine here how far such assertions are warranted, and to enlarge on the degree of improvement to which medical inquirers and teachers among them have attained in the several departments of medical knowledge, or to inquire where the desired revolution in professional opinion concerning those important subjects is more likely to originate. It will be sufficient to remark that, in regard to the matter more particularly before us, professional investigators—the enlightened portion of them, I mean—so far from being sadly behind the times, are, as every unprejudiced reader must know, far ahead of their predecessors; and that nothing has been said or done by contem-

porary writers, in this country or elsewhere, to disprove the correctness of their conclusions. It is, indeed, precisely in reference to the very subjects specified, that the science seems to me to have mostly improved. An examination of professional records will show that, in every place where the opportunity of carrying on the investigation on a sufficiently enlarged scale, and with proper minutiæ and thoroughness, has presented itself, and where men possessing the talents and knowledge requisite to study and extend the stock of information, and to lay the result of their inquiries effectively before the public, have set to work, the pathology, etiology, and therapeutics of fevers and febrile diseases (including parenchymatous and membranous inflammations) have been diligently and successfully studied, and our knowledge respecting them has been very greatly enlarged.

The knowledge thus accumulated has been very freely laid before the medical public, and through means of the writings of those pioneers in the cause of science and of their followers, every reading physician or teacher has the opportunity, if he thinks fit to embrace it, of becoming fully informed as to the matter in question. If many, therefore, among us and elsewhere, are found to be truly amenable to the charge of being sadly behind the times, the fault necessarily lies with them and no one else. They constitute, at best, only a portion—I hope a minority—of our professional brethren. Hence, there can scarcely be any more truth or justice in the assertion that medical men, and professional teachers generally, must find a place in this category of ignoramuses; that they have all lagged a good way behind the times; that, therefore, their opinions should be discarded on the score of their being antiquated, and no longer on a level with the state of knowledge on the subject, than there would be foundation for the idea that the medical inquirer must turn, for truly correct notions regarding the pathology, causation, or treatment of fevers, to the lucubrations of the above few. The assertion, I say, is not founded in justice, nor on solid ground; for the opinions thus impeached are the result of a considerable amount of correctly observed facts, and are based on philosophical deductions of the strictest kind; and should, as such, be respected till disproved by others founded on equally well observed and equally numerous data. It is certain, indeed, that, so far from the whole system of pathology, &c. of fevers and febrile diseases being founded on preconceived notions, and not upon facts and

inductions, a careful survey of what has been done on the subject in France, England, Germany, and the large cities of this country, will show that the views at present entertained are based upon nothing but facts and legitimate deductions, to the exclusion of preconceived notions; that there never was a period when the enlightened portion of medical men were less disposed to be biased by such notions, and when they have less allowed themselves to be led astray by fanciful speculation in their conclusions on pathological and etiological questions than at present; and that, in fact, such a cutting loose from the shackles of preconceived notions constitutes the distinctive characteristic of the present mode of investigation as regards the nature, the cause, the mode of progression, and the treatment of fevers and febrile diseases, and, indeed, of every other morbid ailment. The doctrine of Broussais, erroneous as it may appear to many at the present day, was based on important facts, often well explained, and leading to correct or plausible deductions. The attentive readers of his *Phlegmasies Chroniques*, of his first *Examen*, and of his *Commentaries*, will not gainsay me in this. The doctrine was faulty, not in the deficiency of the facts upon which it was based, but in the nature of many conclusions drawn from them. But how was it overthrown? Certainly, not through the operation of a greater power of reasoning possessed by Broussais's antagonists, for some of the most successful among them are not particularly gifted on that score, but by facts more numerous, more carefully observed and analyzed, and more accurately compared with each other. How have we acquired our present knowledge of the pathology of typhoid fever? Open the works of Louis, Andral, Chomel, Jenner, Gerhard, Flint, and see whether facts are wanting. How have physicians attained their present views of the pathology and anatomical characters of pulmonary or cardiac diseases? How have they learned to ascertain, by means of auscultation and percussion, the condition of internal organs? Is it by abstract reasoning, or by following out and cherishing some exclusive and dominant idea, and forcing everything to bend to it; some preconceived notions; or is it not rather through the instrumentality of facts accumulated in France, England, Germany, and a small portion of this country? It can scarcely be necessary to answer the question.

Those who raise such an outcry about the necessity of facts, and reprove the most authoritative writers and teachers for a supposed adherence to preconceived notions, and a neglect of the true prin-

ciples of the inductive philosophy, should first satisfy the profession, not only that they are less liable to error than their opponents, in drawing deductions from the facts they may happen to collect, but that these facts have been examined in a way calculated to insure advantageous results. They should remember that, great as the number of useless theories may be, it sinks into insignificance when compared with the amount of incomplete facts which float around us. To observe accurately—to seize the important and useful points in the case examined—to discover its true bearing to other cases, its exact analogy to or dissimilarity from them—to ascertain its dependence on, or independence of, surrounding influences—to point out accurately the morbid agencies which have produced it, and to ascend by an analysis of its symptoms and anatomical characters, to its pathological nature and therapeutical indications, require a degree of skill, a tact, a habit of close observation that are not easily acquired, as well as opportunities for investigation that do not fall to the lot of every one. And yet a writer must give proof of possessing them before he can expect to command the attention of the cautious inquirer, and change the current of professional belief. "A vast mass of facts," says a recent writer, "may be collected and tabulated—the numerical method may be applied in every conceivable mode—and yet, so long as no connecting idea is discovered among them, they remain utterly incapable of serving for the establishment of those general principles which enable true science to predict with certainty what will not occur in any given contingency; and which, therefore, furnish the only satisfactory basis for the use of art as to what should or should not be done." A great master, Montesquieu, has said: "Un fait qui n'emporte pas une idée, duquel on ne peut pas s'élever pour voire plus loin, est un caillou qui ne vaut pas la peine d'être ramassé, et qu 'il faut au contraire repousser du pied pour en débarasser la route."

What the result of the mode of proceeding adopted at this day by standard pathologists has been as regards the distinctive pathognomonic marks, and the separate nosological positions of the several forms of fevers, properly so called, has already been stated. Much the same may be found in reference to other febrile complaints dependent on local inflammation, as well as to other diseases; to the symptoms and anatomical characters by which they are recognized, and which distinguish them from each other and

from the pyrexiæ, as well as to the causes by which they are produced. Now if in the course of this revolutionizing movement, and at the present period of progressive improvement, some medical writers or teachers are really found to be a long way behind the times, it can scarcely be those who have taken an active part in the race of investigation, who have helped to overthrow old and erroneous views, and, by appealing to facts, to place matters in a proper light, or who have diligently and carefully studied the works of modern standard writers. They must be sought elsewhere.

Here I must close the remarks I wished to offer in opposition to the supposed connection between pneumonia and periodic fevers. They have extended far beyond what was originally intended. For this the reader is entitled to many apologies. The greater part of the volume thus inflicted on him was penned somewhat in haste, and under the pressure of a variety of circumstances unfavourable to minute investigation, systematic arrangement, and correct composition. With more tranquillity around me, with a larger share of health than I have recently enjoyed, with a freer and more constant access to publications relating to the questions at issue, and especially with more leisure at my command, it might have been presented in a shape more acceptable to those who will do me the honour to peruse it. But I must plead as an excuse, the same reason which Voltaire assigned for having written a very long letter to one of his princely correspondents—I had not time sufficient to write more briefly. Such as it is, with all its many imperfections and gross defects of commission and omission, these remarks are placed before the medical public in the hope that they may do some service, and contribute in some measure to the settlement of the question; or at any rate that they may excite in others, better qualified for the task, a desire to point out, in an ampler, clearer, more logical, and more forcible manner, the groundless nature and dangerous tendencies of the pathological and ætiological heresies to which I have called attention.

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